SCIENCE

FRIDAY, FEBRUARY 4, 1898.

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MSS. intended for publication and books, etc., intended for review should be sent to the responsible editor, Prof. J. McKeen Cattell, Garrison-on-Hudson, N. Y.

*THE BIOLOGICAL PROBLEMS OF TO-DAY.**

Paleontological Problems. PROFESSOR HENRY F. OSBORN, Columbia University.

THE chief paleontological problems of the present day are involved in the phylogeny of the Mammalia, for upon this depend both Embryology and Comparative Anatomy, as well as Paleontology. The last decade has been one of a rapid succession of brilliant discoveries in South America and in southern Africa, and of a very great expansion of our knowledge of the North American fauna, together with some single discoveries of great importance, chief among which is the discovery of the foot structure of *Psittacotherium* by Wortman, leading to his exposition of the order Ganodonta as ancestral to the Edentata. Of great interest also is the hypothesis recently advanced by Matthew, that *Mixodectes*, of the Basal Eocene, is the ancestor of the Rodentia, instead of being connected with the Primates, as Cope supposed.

As regards the South American forms they are mainly important as revealing the existence of a new life center upon a continental scale; as tending to demonstrate a continental union between South America and Australia, and as exhibiting Marsupials which are more nearly allied to Placentals than any hitherto known. As Lydekker

*Discussion before the annual meeting of the American Society of Naturalists held at Ithaca, N. Y., December 28, 1897.

and Scott, on paleontological grounds, and Hatcher, on geological grounds, have demonstrated, this fauna is by no means of the great antiquity assigned to it by Ameghino. It is rathern modern and specialized than central and ancestral. The sources of this fauna should be sought possibly in an overflow of primitive Marsupials from Australia or elsewhere, and partly in an early emigration of Condylartha from North America. The latter may have constituted the origin of the Litopterna, but they give us no light upon the Toxodontia. At the same time the general principle of North American origin is strongly reinforced by the demonstrated relationship of the Ganodonta to the Edentata.

As regards the remaining Ungulates of the world, the origin of the Proboscidia and Hyracoidea is still wholly unknown. The Sirenia also remain without known ancestors. The group of *Ancylopoda*, proposed by Cope for *Chalicotherium* and other clawed forms with the bodily proportions of the Sloths, but many essential skeletal structures of the Perissodactyls, loses its distinctness from the Perissodactyl phylum, because of the discovery that *Agriochoerus* besides *Diplobune*, both undoubted Artiodactyls, exhibit a very marked parallel specialization of hoofs into claws. Going further back to the Lower Eocene there still exists a break between the Artiodactyla and Perissodactyla and any of the known forms of Condylarthra, for none of the latter are as yet proved to be directly ancestral to the even or odd-toed Ungulates. The Amblypoda stand apart as a very ancient and distinct phylum, geologically the oldest, and in structure the most archaic of all Ungulates; they should include the *Periptychidæ* and thus embrace the whole range of amblypods from the small arboreal Periptychids to the huge clumsy Uintatheres.

The most primitive type of Condylarth (*Euprotogonia*) and of Amblypod (*Pantolambda*), as recently studied by Osborn and Matthew, strongly reinforces the hypothesis first enunciated by Cope, *that the source of the Ungulata is to be found in the Creodonta.* Upon the other side of the great Mammalian tree, the numerous branches of Unguiculates or primitive clawed types also have converged towards a Creodont ancestry, as seen especially in the characters of the Ganodonta, or ancestral edentates, and of the Rodentia, if Matthew's supposition proves to be correct; also of the Tillodontia. Thus all these groups should probably be added to the Carnivora as Creodont derivatives. The Carnivora extend back into Creodont prototypes; but, as in the case of the Artiodactyla and Perissodactyla, the actual points of contact or links between these two divisions are yet to be discovered. So, again, with the Primates. Recent embryological evidence has tended to separate the Lemuroid and Anthropoid phyla. Hubrecht is confirmed by others in placing *Tarsius* near the parting of these phyla (although not in his separation of this genus from the Lemurs), and he makes a very radical break between Lemurs and monkeys upon grounds of placentation. The point of contact of the Primates with the Creodonta is still entirely wanting, but their relations appear to be here rather than with the Insectivora.

In spite, therefore, of the many remaining deficiencies or absence of links in our paleontological evidence, it has none the less come about *that the Creodont type takes the central position which was assigned by Huxley in 1880 to the Insectivora,* for the known Creodonta are more generalized and more central than any other of the known Insectivora, fossil or living, the known Insectivora showing a very considerable specialization, especially in their dental succession, which places them apart as a distinct side phylum. This does not affect the derivation of the Creodonta themselves from stem forms of unspecialized Insectivora existing in

the Jurassic period, the characters of which are very largely seen in the *Insectivora Primitiva*, or placentals of the Stonesfield Slate and Purbeck periods.

The discoveries in South Africa above alluded to take us back to the still older period of the origin of the Mammalia. Two of the types of the Theromorphs of the Permian and Lower Triassic, namely, the *Theriodontia* and *Gomphodontia*, supply many of the characters which we have expected to find in the ancestry of the Mammals. In fact, they embrace the few osteological characters placed in Haeckel's Promammalia, or Huxley's Hypotheria, as well as the more numerous characters which we have subsequently put into the Mammalian archetype. The *Theriodontia* resemble in their dentition and structure the minute *Protodonta* described by Osborn from the Triassic, but differ in the compound character of the jawbones as well as in their surpassing size. In tooth structure they are also prototypes of the *Triconodonta* or Marsupials of the Jurassic period. On the other hand, the herbivorous *Gomphodontia*, including *Tritylodon*, are prototypes of the great phylum of Multituberculata, which in turn, upon extremely slender evidence however, have been associated with the Monotremata.

Thus while the phylogeny of the Mammalia is still in a highly incomplete, speculative and shifting condition, if compared with the evidence we could have mustered ten years ago, it marks a prodigious advance and is full of stimulus for the immediate future of paleontology.

Botany. PROFESSOR WM. TRELEASE, Missouri Botanical Garden, St. Louis, Mo.

THOUGH for a time I found opportunity for work along ecological lines, necessity has compelled me to confine my study, of recent years, so closely to descriptive botany that at first I felt some hesitancy in accepting the invitation to open this discussion of the biological problems and proposed methods for their solution, in botany. But, on second thought, I decided that I might, without impropriety, do so, since I recalled the statement, heard many years ago on this campus, that the ultimate systematic arrangement of living things will be at once an epitome of all that is known of them and a key to their entire history; and I fully recognize that many of the most serious problems confronting the descriptive naturalist to-day are to receive their solution through increased knowledge of the things studied as living things. In point of fact, the great problem for the botanist and zoologist, the problem underlying and running through all others, is the problem of life.

I have seen so many vital phenomena explained by normal, if complex, physical laws that I may be pardoned, I trust, if at the outset I state that I look at this problem as a physicist and not as a vitalist, feeling that, with each added physical demonstration given, the improbability of an extra-physical answer to each unanswered question becomes in an even greater degree unlikely.

That which the botanist and zoologist are primarily concerned with is protoplasm. In general essentials alike in animal and plant, yet in detail differing in two individuals of the same species, in the twin offspring of the same parent, in different organs of the same organism, and seemingly in the same living cell at different periods, differentiated so that, at least in the vegetable kingdom, the morphological unit, the cell, is yet a complex organism, this substance represents apparently a most complex and ever-changing mixture of most complex and unstable organic compounds.

Though the animal possesses a higher specialization and a greater corresponding differentiation of its cells, and though those

which are exposed, or upon which much devolves, are here bound together by a wonderfully developed class of nerve cells, along which, from the center of sensation, travels an impulse which, through terminal dendrites, may establish and re-establish itself in that wonderful phenomenon which we call memory, and though the metabolic processes connected with the maintenance of animal temperature and with nerve structure and nerve action are more complex and less differentiable than in the vegetable kingdom, so that the plant is frequently turned to for an illustration of simple cell action; the green cell performs that added function of photosynthetically recombining the elements of simple unassimilable compounds into assimilable organic compounds, which by specialized structures are converted into organized substance, which again, by the action of secreted enzymes, is digested and fitted for transportation to points where it may be wanted for use, while these same compounds are still further synthetized by the incorporation of nitrogen, for the most part in relatively simple organic combination, so that it is by no means certain that the simplest field for the study of protoplasmic activity is afforded by the plant. Here, then, in the nutritive changes induced by and occurring in this delicately balanced vehicle of vital manifestations, lies the seat of one great problem: Is life life, or is it an attribute of matter? Does the synthesis of organic matter stop with the formation of the vegetable carbohydrate or the vegetable reserve proteid, or does the one pass into the other, which in its turn grades into the living protoplasm of the cell, the molecules of which, during active life, undergo continuous mutations and shifting combinations from the nutrient to the living and from the living to the excreted form? A part of this question has been answered. What shall be the answer to the other part? and if physical, what is to be said as to a positive suspension of protoplasmic activity, amounting to functional death, and of a revivification of protoplasm which actually has been dead?

One must concede that in plants, as in animals, death inevitably comes, sooner or later—unless one chooses to juggle with terms in an effort to prove that the unicellular organism, the individual cambium cell and the like are immortal. But in what does it consist?

In medicine a system of pathology has been worked out by which the theory and practice of a generation ago have become the science and art of to-day. For plants a science of pathology just as complicated, just as useful for the preservation of the life of the individual, remains to be worked out. Does disease cause 'loss of vitality,' or is this merely an expression of imperfect nutrition or clogging by waste products? What is anæsthesia? Is it a temporary reduction of vitality in certain cells, or an enveloping of their molecules by the inhibiting agent or its derivatives?

What is reproduction? What is heredity? The vehicle of each, as of every other vital phenomenon, is protoplasm; more, it is known that nuclein is directly concerned in the reproductive processes, and the technique of to-day has enabled it to be shown, for plants as for animals, that certain parts of certain cells unite. The physical or visible basis for a theory of the transmission of characters is more nearly reached to-day than ever before, but is the real essence of the problem any nearer elucidation? Why does the fertilized gamete of the alga produce a seaweed, and of the phanerogam a flowering plant? Why does the meristem of the oak produce oak leaves on all branches?

An analysis and subanalysis, to the last degree, of all of those phenomena which we call vital, and a chain of experiments elim-

inating successively each of the factors which can affect any vital process, can alone give answer. We may not live to see it, but perhaps it is not impossible that, though not a spontaneous generation of organisms, a planned generation of living matter may be effected under the eye of the experimenter.

Of the grosser directly biologic problems facing the botanist, none is more simple in appearance, nor apparently more difficult of solution, than that attending the rise of crude sap from the root to the leaf of one of the higher plants. Purely physical in the wonderful osmotic action of the absorbent cells, and purely physical in the evaporative action of the foliage, the flow of sap has a middle part to which the laws of physics elsewhere have not been fitted; and yet this conduction in the main is carried on through tissues which are dead. Here, too, the isolation, one by one, of all disturbing possibilities offers the only control of experiments from which final conclusions are to be drawn.

The plant has not a nerve system. It is true that its protoplasm communicates from cell to cell through all of the living parts, but no differentiated chain of corpuscles exists for the transmission of sensation, or whatever else you choose to call it, from organ to organ, much less from operative organs to a central control organ; and yet there are plants which are called irritable or sensitive; organs which, if touched, coil about a support—clasp, for digestive purposes, prey; leaves which, for protective purposes, drop into an inconspicuous position, or into a position exposing them less to the heat of the mid-day sun or radiation into the cold of night. These movements are said to be reactions to stimuli, manifestations of protoplasmic irritability; but those who have looked deepest into them find the difficulties of explaining the exact process multiplied the further they go. Division of the problem, division of labor, experimentation and observation under conditions most favorable for the normal growth of the plant, are the means of reaching a solution.

We owe it chiefly to Darwin that a science of ecology has sprung into life. The German school would call it biology, but it is not precisely what is immediately considered here as biology. It is the interrelations between living things and between them and their surroundings. All that, with loose expression of teleological purpose, would be called 'adaptation' belongs here. Many facts are well known. The theories advanced for their explanation often seem to explain them, but the theories concerning their origin are not always so satisfactory. Who can say that with more knowledge we may not discard even the most fundamental of them? Observation and differential experimentation are here means to the end, no less than elsewhere. How do plants react to their surroundings in nature—under cultivation? How have their species come into existence in their present form? The general fact that they do react, and that they have been evolved from preëxistent types by a process in some degree of the survival of the fittest, is currently believed. The horticulturist to-day produces what he openly calls species in the vegetable kingdom. Are not his methods indicative of the line to pursue in answering the more recondite questions of descent and multiplication?

In conclusion, to come to that in which more nearly I myself am compelled to work, I wish to state that the study of local floras—the study of the flora of one's back yard in a city, of his stone wall, of the roof of his house, if we have an old house, of an old cheese-box—is far from being a mere determination and enumeration of the several species represented. It is becoming a census of the individuals, an

investigation of the communities that they form, and of the interlocking of these into greater, more complex communities; a study of the external configuration of individuals, with reference to their resistance to undue humidity, undue dryness, unusual cold, extreme heat; an anatomical study of their several organs as connected with the same factors; a chemical study of their secretions in the same light; and, finally, a return to that with which I began, a study of their protoplasm in all its phases.

Anatomy: What is the Morphologic Status of the Olfactory Portion of the Brain? Professor Burt G. Wilder.

In view of the multitude of problems now confronting anatomists,* it has seemed to me that the present occasion may be best utilized by discussing, in some detail, a single topic which has, nevertheless, intimate relations with several others in anatomy and embryology, human and comparative. Most of the points are indicated upon the wall-maps exhibited.†

Stated more specifically, does *the olfactory portion of the brain constitute a definitive segment; or does it, together with the striatum and pallium, constitute merely the 'dorsal zone' of a segment whose ventral zone is the 'pars optica hypothalami,' i. e., the region about the chiasma?*

As a basis for the consideration of this question are offered the following propositions, the validity of which each must determine for himself:

1. We must distinguish between the *potential neuromeres*, the precise number of which may not be determined for decades, and the *definitive segments*, which are convenient and natural divisions, even if not all of equal morphologic value.

2. For the determination of the segmental constitution of the brain more reliance is to be placed upon comparative anatomy and embryology than upon the structure and development of that morphologic monstrosity, the human brain.

3. The recent enactments of the Anatomische Gesellschaft upon this subject (B. N. A., 1895) are based almost exclusively upon the conditions in a single member of the vertebrate community, man; at the best, even if they apply more or less closely to the other mammals, they constitute an example of 'class-legislation.'

4. When a writer employs a term in a sense other than either (*a*) that which is generally accepted, or (*b*) that in which it was first introduced, or (*c*) that in which it is used by other writers whose views he may be discussing, it is incumbent upon him to state explicitly the sense in which he proposes to use it.

The present obstacles to the recognition of a rhinencephalic segment are three, viz.: (1) The common impression as to the insignificance of the olfactory region. (2) The existence, in the higher vertebrates, of the modification designated by me as the diencephalic flexure. (3) The adverse view adopted in the B. N. A., based largely upon the assumption that the region

*In 1894 I stated (Records of the Association of American Anatomists, sixth meeting, p. 32) that, in addition to about fifty special questions respecting each of the fifty particular cerebral fissures, there are at least one hundred general problems connected with them as a group of features of what is commonly mentioned as a single organ.

†These included diagrams of the brains of man, sparrow, turtle, *Necturus*, *Ceratodus*, *Scymnus* (after T. J. Parker), *Chimæra*, *Polyodon*, *Petromyzon* and *Bdellostoma*: a diagram of the mesal aspect of the human thalamus, *etc.*, exhibiting the location of the aulix ('*sulcus Monroi*') as first described by Reichert, together with the deflection of its cephalic half as proposed by His; and schemas representing (*a*) the dorsal aspect of the six definitive segments now recognized by me; viz.: Rhinencephal, Prosencephal, Diencephal, Mesencephal, Epencephal, Metencephal; (*b*) the same as if medisected; (*c*) the several brain flexures, especially the diencephalic; (*d*) the five different topographic relations to the general axis of the brain (as represented by the olfactory crus) of the presumed psychic expansions.

cephalad of the mesencephal comprises dorsal and ventral zones demarcated by an alleged sulcus connecting the mesocele with the *recessus opticus*.

1. Doubtless all members of this society have discarded the anthropotomic estimate of the olfactory bulbs and their crura as constituting merely a 'first pair of cerebral nerves.' But not all, perhaps, fully realize that, notwithstanding their complete absence in certain adult Cetacea, in most Mammals the olfactory bulbs are quite massive; that in Batrachians, Reptiles and most Selachians they constitute a large proportion of the brain; and that in lampreys and hags they equal in size 'the cerebral hemispheres.'

Had the study of the vertebrate brain begun with Myxine or Bdellostoma the olfactory bulbs would have been unhesitatingly assigned a rank at least equal to that of either of the three following subdivisions.

Whatever the ontogeny in a given case, it is probable that phylogetically the smelling portion of the brain preceded the reflective.

"The revolution, so to speak, of the 'hemisphere' about the olfactory axis accords with other considerations which have led Spitzka and the writer independently to consider the prevailing idea that the olfactory lobes are mere appendages of the cerebrum as nearly the reverse of the truth."*

2. The Diencephalic Flexure. With Reptiles, Birds and Mammals, the forms with which most anatomists are more familiar, the first (cephalic or 'anterior') of the series of cavities seems to be the '*ventriculus tertius*'; indeed, in some Birds and Mammals the recess at the root of the optic nerve actually lies farthest cephalad. This condition seems to be associated with the general crowding of the cerebrum dorsad and caudad over the other parts of the brain. It is discussed briefly in the *American Association Proceedings*, 1887, pp. 250–251; *American Naturalist*, October, 1, 1887, 914–917; Reference Handbook of the Medical Sciences, VIII., 112, and *Journal of Comparative Neurology*, VI., 128.

* The Dipnoan Brain, *American Naturalist*, June, 1887, p. 546.

The following propositions seem to me warranted by the conditions in Batrachians and 'fishes:'

However numerous or sharp the dorso-ventral flexures of a given brain, for comparison with other brains or with an ideal schema the axis is to be regarded as straight.

Whatever its actual position, the aula or mesal space between the two portas ('foramina of Monro') constitutes the cephalic member of a longitudinal series of cavities.

From the standpoint of comparative neurology the terma ('*lamina terminalis*') is a constituent of the floor of the encephalic cavities; its dorso-ventral position in Reptiles, Birds and Mammals no more converts it into a morphologic end-wall of those cavities than its dorso-caudal inclination in certain forms entitles it to be interpreted as a portion of the roof.

3. In order to be entitled to rank as a definitive segment must a given region exhibit the dorsal and ventral zones of His?

Conceding, for the present, the constancy and significance of these zones in the myel (spinal cord) and in the brain as far as the cephalic orifice of the mesocele ('aqueduct'), are they represented in the region beyond?

In the absence of complete developmental and histologic evidence on that point, my provisional answer in the negative is based upon two very different considerations:

First, the general distinctions between the parts derived from the first encephalic vesicle and the rest of the cerebro-spinal axis. *Secondly*, the unsatisfactory presentation of

the subject by those who attach most importance to it.

In 1859 and 1861 Reichert described and figured (Der Bau des menschlichen Gehirns, Plates II., X., XI., p. 65, line 5) a furrow on the mesal aspect of the thalamus, connecting the 'aqueduct' with the *foramen Monroi*. To this he applied the name *sulcus Monroi*, which has been generally employed. In 1884 the mononym *aulix* was proposed by me, and the feature has been shown distinctly in the *New York Medical Journal*, March 21, 1885, p. 327, and 'Reference Handbook,' Vol. VIII., p. 122, and IX., Fig. 418.

In his exposition of the schema adopted by the Anatomische Gesellschaft (B. N. A., pp. 157–159) Professor His insists upon the great morphologic significance of the dorsal and ventral zones, and of the '*sulcus limitans ventriculorum*'* by which they are demarcated. He further declares that the continuation of this sulcus is the *sulcus Monroi*. But his figures represent the sulcus as terminating, not, as with Reichert, at the *foramen Monroi*, but at or near the optic recess, and, without explanation of the radical deflexion, he says, "Die Sulci Monroi laufen jederseits im *Recessus opticus* aus." The confusion caused by this unspecified transfer of a title to a different feature is augmented by the account of the same matter by C. S. Minot in the *Popular Science Monthly*, July, 1893; here the text is explicit as to the importance of the sulcus and its termination at the *foramen Monroi;* but the figure represents the boundary between the zones at a point farther caudad.

In this connection it should be stated that the recent studies of Mrs. S. H. Gage upon embryo cat, turtle, batrachian and bird (*Amer. Nat.*, October, 1896, 837) have revealed sulci having various directions, but not, apparently, demarcating the dorsal and ventral zones.

*For this I have proposed the more definitely correlated name *sulcus interzonalis*.

In view of the present aspect of the case, while I see no impossibility in the representation of the dorsal and ventral zones in the first three segments of the brain, and while such zones might well be demarcated by the furrow originally described by Reichert as '*sulcus Monroi*' (my *aulix*), I hold that the interpretation of the olfactory portion of the brain as merely one part of the dorsal zone of a segment must be supported by something more than the designation of a limiting sulcus which is apparently either non-existent or without interzonal significance.

Psychology. Professor J. McKeen Cattell, Columbia University.

The speaker said that the knowledge of paleontology, reasonably presupposed by Professor Osborn on the part of all students of natural science, could scarcely be expected in the case of psychology. Neither was it possible to exhibit the whole of psychology on a single blackboard, as Professor Osborn had done for paleontology, or even in a more bewildering series of charts, such as Professor Wilder had found needful for neurology. He could only make some very general, and, he feared, somewhat trivial remarks.

Each science has problems in common with other sciences and problems peculiarly its own. We who are trying, each of us, to advance some little department of science cannot but sometimes stand at gaze before the magnitude of modern science. How can we see the forest for the trees, the library for the books, the world for the facts? Professor Klein has said that mathematics is ten thousand years in advance of the other sciences, but how does he know whether the sciences are an asymptote to his mathematics or whether mathematics are going off on a tangent to the rest of the universe? Professor Klein tells us that to the regular polygon of 65,537

sides Professor Hermes has devoted ten years of his life. It was once a vital question as to how many angels could dance on the point of a needle. Apart from the earmark of material utility, it is not easy to adjust scientific values. We trust that in religion, in art and in science there is, in addition to the transient, the permanent. But it is a problem, and a difficultone, for the soldier in the thick of battle to reflect on international law and constitutional history.

The magnitude and the multiplicity of science suggest a problem that has always been emphasized in this society. Each of us is a teacher:

"And gladly wolde he lerne and gladly teche."

But what shall he learn and what teach, what forget and what ignore? Admitting the narrow capacity of a single mind, with what shall it be filled? Each with diverse contents, doubtless, if we are to secure the best results. But what shall be the common property of all—what should we learn and teach in school and college? Certainly none here can ignore the doctrines of evolution; probably none should neglect the fundamental concepts of physical science; perhaps we should all know how to use a tool as fine as the calculus. But should a large part of the six or eight years of greatest receptivity be given to Latin and Greek? It is a difficult question. The classics, in our present civilization, are a mark of culture that no one likes to be without. But are they the causes of culture, or only its insignia? Are they to be classed with white linen and polished shoes, possibly even with tight lacing and high heels, or do they give us more life and better?

Turning now to the problems concerning the content of the biological sciences, I venture to maintain that the science of today is either quantitative or genetic. Modern physical science is scarcely older than the doctrine of the conservation of energy —50 years old. Modern biological science may properly date from 1859. The physical sciences then became quantitative, and the biological sciences then became genetic. Earlier, the sciences were largely engaged in giving things names. The zoologist, the botanist, the psychologist, and even the physicist had the naïve faith in names as a method of description of the little girl who remarked that Adam had given a very appropriate name to the hog. We still, I fancy, have a somewhat exaggerated confidence in laws, theories and animistic personifications, as explanations of the development of living things. I believe that the great problem now before biological science is to add to its genetic method the quantitative method of physical science, and thus apply a kind of description, economical and far-reaching beyond all others.

Yet, here another problem arises. When we have our quantitative and causal science, our formula bears about the same relation to the world that it is intended to express as a herbarium does to a primeval forest. Our regard for the body of nature becomes that of the anatomist rather than than that of the lover. How can we reduce things to an abstract formula without ignoring their concrete and infinite variety? Fortunately, the subject of this discussion is the biological problems of to-day, not their solution.

As to the problems peculiar to the psychologist, it would be scarcely becoming to bring our family quarrels before the larger public of the biological sciences; besides, they are too numerous to be even mentioned in the latter part of ten minutes. I do not like the term 'the new psychology.' I prefer to maintain that psychology is one of the oldest of the sciences. Still, if modern physics is only 50 years old, and modern biology only 40 years old, modern psychology is still younger. I am not as old as I

expect to be some day, but I was, I think, the first professor of psychology as a separate subject, not only in America, but anywhere. When our present psychology is so young, it is natural that there should be difference of opinion, and even confusion, in regard to its scope and methods.

Our great problem, it seems to me, is the one I have already mentioned as common to all the biological sciences—the extension and coördination of the genetic and quantitative methods. And we have really accomplished a great deal. There was no laboratory of psychology in America, and only one in the world, prior to 1883. Now they are everywhere—perhaps forty in American colleges and universities. In nearly all these laboratories experiments are in progress, which are enlarging our knowledge of sensation, of movement, of feeling and of action. Parallel with this development of experimental psychology, bringing our science into fruitful relations with the physical and mathematical sciences, there has been a noteworthy advance in genetic psychology—witness the address of the President of our Association this year—placing mental development in close touch with all the biological sciences. At the same time increased knowledge of the relations of body and mind has made almost a special science of physiological psychology. Degeneration is a phenomenon common to all the biological sciences, but unfortunately one very prominent in the subject-matter of psychology. Here we have a wide field with many points of contact with pathology and medicine. In the interrelations of minds we cross the paths of anthropology, of sociology, of philology and of history. Psychology is concerned with art and with conduct; it is essential to a sane philosophy.

The subject-matter and the problems of psychology are entangled with those of many sciences, but perhaps with none so closely as with those represented in this discussion. We students of psychology need to know what you are doing, and welcome as a help this affiliation of societies. We hope that you in turn will find that psychology should not be neglected, but that it contributes something to each of the biological sciences and to the advancement of science as a whole.

Physiology. Professor Jacques Loeb, University of Chicago.

If it be true that the fundamental problem of Physics is the constitution of matter, it is equally true that the fundamental problem of Physiology is the constitution of living matter. I think the time has come for Physiology to return to its fundamental problem.

Living matter is a collective term for the qualities common to all living organisms. Comparative Physiology alone enables us to discriminate between the general properties of living matter and the functions of specific organs, such as the blood, the nerves, the sense organs, chlorophyll, etc. Nothing has retarded the progress of Physiology and Pathology more than the neglect of Comparative Physiology. Comparative Physiology shows that secretion is a general function of all living organisms and occurs even where there is no circulation. Hence it was *a priori* false and a waste of time to attempt to explain secretion from the experiments on blood pressure. Oxidation occurs regardless of circulation, and it was *a priori* a waste of time to consider the blood as the seat of oxidation. Comparative Physiology has shown that the reactions of animals to light are identical with the heliotropic phenomena in plants. Hence it is a mistake to ascribe such reactions as the flying of the moth into the flame to specific functions of the brain and the eyes. Sleep is a phenomenon which occurs in insects and plants, and it would

be a waste of time to attempt an explanation of sleep on the basis of phenomena of circulation. The best interests of Physiology and Pathology demand that the systematic development of Comparative Physiology be one of the physiological problems of to-day.

May I be pardoned for calling attention to one special field of Comparative Physiology which I believe to be especially fertile? I refer to the field of Physiological Morphology. I applied this name to the investigation of the connection between the chemical changes and the process of organization in living matter. Two series of facts allow us to connect these two groups of phenomena: (1) The fact that phenomena of fermentation lead to an increase in the number of molecules, and thus bring about an increase of osmotic pressure in the cells. This increase of osmotic pressure is the source of energy for the work of growth. (2) The facts of heteromorphosis, *i. e.*, the possibility of transforming in certain animals one organ into another or substituting one organ for another, through external influences, such as gravitation, contact, light, etc.

The exact and definite determination of life phenomena which are common to plants and animals is only one side of the physiological problem of to-day. The other side is the construction of a mental picture of the constitution of living matter from these general qualities. In this portion of our work we need the aid of physical chemistry and especially of three of its theories: Stereochemistry, van 't Hoff's theory of osmotic pressure and the theory of the dissociation of electrolytes. We know that the peculiar phenomena of oxidation in living matter are determined by fermentative processes, and we venture to say that fermentations form the basis of all life phenomena. It has been demonstrated that fermentability is a function of the geometrical configuration of the molecule. Saccharomyces cerevisiæ is a ferment for such sugars only as have three, or a multiple of three, atoms of carbon in the molecule. Among the Hexaldoses only d-glucose, d-mannose and d-galactose are fermentable, while their stereoisomeres are not fermentable. But the influence of the geometrical configuration goes farther. Voit has suggested, and Cremer has demonstrated, that there is a far-reaching parallelism between the fermentability and assimilation of Carbohydrates. Higher animals as well as yeast cells are able to form glycogen from such carbohydrates as are fermentable by yeast. The further development of these stereochemical relations and their extension to proteids and nucleins is another of the problems of Physiology which will contribute to the main problem, the analysis of the constitution of living matter. I believe that the influence of stereochemistry will be more or less directly felt in many branches of Physiology, in questions of heredity as well as in the theory of space sensations, as E. Mach has already intimated.

Van 't Hoff's theory of osmotic pressure permits an application of the law of conservation of energy to a class of phenomena to which this law was hitherto inapplicable, namely, the phenomena of growth, functional adaptation, secretion, absorption and even pathological processes, such as œdema. The physiologists who thought that the blood pressure determined secretion could not understand why secretion took place under a higher pressure than the blood pressure. Comparative Physiology shows that secretion does not depend upon circulation, and the theory of osmotic pressure indicates that the osmotic pressure in the cells is more than twenty times as high as the blood pressure. The work of secretion is done by osmotic pressure, and not by blood pressure. A

prominent physiological chemist has become a vitalist because he could not explain why the secretions differ from the blood from which he thinks they are formed. He overlooks, among others, the fact that the protoplasm possesses the quality of semipermeability, which means that it allows certain substances to pass through, and others not. In my opinion, the working-out of a theory of semipermeability is one of the main physiological problems of the day.

The theory of the dissociation of Electrolytes is of fundamental importance in the analysis of the constitution of living matter. Pharmacology will feel its influence most directly. Everything seems to indicate that the specific physiological effects of inorganic acids are due to the number of positively charged Hydrogen Ions in the unit of solution, and the specific physiological effects of alkalies to negatively charged Hydroxyl Ions. But the universal bearing of the theory of dissociation upon Physiology will perhaps be best seen in the field of animal electricity. An active element of living matter is negatively electric to its surrounding parts. We may assume that an acid is formed in the active part, and that the passive parts are neutral. The positive Hydrogen Ions of the acid have a much greater velocity of migration than the Anions. Hence the former will diffuse more rapidly into the passive tissue than the Anions, and the active tissue will remain negatively charged.

At no time since the period immediately following the discovery of the law of conservation of energy has the outlook for the progress of Physiology appeared brighter than at present. But in order to reap the full benefit of our opportunities we must bear in mind that the fundamental problem of Physiology is the determination of the constitution of living matter, and that in order to accomplish our task we must make adequate use of Comparative Physiology as well as Physical Chemistry. Pathology, in particular, will be benefited by such a departure.

Developmental Mechanics. PROFESSOR T. H. MORGAN, Bryn Mawr College.

IN the last few years a new movement has started in embryology known as Entwicklungsmechanik, developmental mechanics, or rather the mechanics of development. In the few minutes at my disposal I shall try to show:

I. How the term Entwicklungsmechanik arose and how it has been defined.

II. I shall try to give an idea of the kind of work that has been done.

Roux, in 1885, first used the word developmental mechanics and defined it as the study of the causal morphology of the organism. It is of importance to note that Roux uses the word mechanics not only in its physical sense, but in its wider philosophical meaning. Therefore, in the definition of developmental mechanics as the study of the causal morphology of the organism, Roux means simply that the changes in form through which the embryo passes are the result of a series of causes, and these causes are what the new study proposes to investigate.

It may seem pretentious to state that this is a new study, for every embryologist must believe that the ultimate goal of his work is to determine, as far as possible, the causes of development. But let us look a little more closely into Roux's position.

Perhaps the problem may appear clearer if we consider it in the form of a concrete example. In what way, for instance, would the study of the mechanics of development differ from ordinary descriptive embryology?

We see the egg segment and then form a blastula, gastrula and larva. Descriptive embryology gives a series of pictures of these

different stages. The more complete the series the fuller will be our knowledge.

Now Hertwig maintains that this knowledge of the successive stages of development is itself causal knowledge beyond which we can not hope to go. He holds that the egg is the *cause* of the blastula and the blastula the cause of the gastrula, etc. Hertwig pretends to be completely satisfied with knowledge of this sort.

Comparison is not perhaps always just, yet Hertwig's position is the same, I think, as though a physicist were to say that if we knew the path of the moon around the earth we should know everything that we could hope to know, or if the astronomer claimed that the position of the moon at one moment is the *cause* of its next position.

Roux, on the other hand, maintains that in order to understand the successive stages of development we must know how the one transforms itself into the other, how the blastula invaginates to form the gastrula, how the medullary plate of the vertebrate embryo rolls in to form a tube. The movements, then, of the parts of the embryo are to be studied. But even a knowledge of the movements of cells and groups of cells would not be causal knowledge, although it might, perhaps, be called the mechanics of the embryo. What makes the endoderm turn in? What induces the medullary plate to roll up into a tube? What, in brief, are the forces at work?

A few illustrations of the kind of work that embryology has already accomplished may bring before us more clearly the problems of to-day.

Pflüger's experiments on the effect of gravity on the segmentation of the egg naturally suggest themselves first. When the egg of the frog is inverted, with its dark hemisphere turned down, the cleavage planes appear, not in their normal position, but in respect to the direction of the force of gravity. At first Pflüger seemed to think that there is some causal relation between the force of gravity and the forces that direct the cleavage of the egg.

Roux showed, however, that a centrifugal force could replace the force of gravity, and, moreover, that if the experiment were so arranged that the centrifugal force *just* overcame the force of gravity then the egg segmented normally in whatever position it was placed.

Finally Born showed that a rotation of the contents of the inverted egg occurred so that the lighter parts rose to the highest points. It is obvious, therefore, that gravity only indirectly affects the egg by bringing about a rearrangement of its contents.

This series of experiments is instructive, I think, in that it illustrates how one experiment leads to another, and how our knowledge of the forces at work is advanced with each well-planned experiment. We do not know, to be sure, why the egg segments, but we have found out something definite about the action of gravity on the egg.

Let us now consider another series of experiments: Roux found that by preventing the development of one of the first two blastomeres of the frog's egg the other uninjured blastomere developed into a half-embryo. Naturally enough, he drew the conclusions that the first two cells are self-differentiating, and that the development is, at least in part, a mosaic work. The conclusion was, I believe, not justifiable at the time, because in the experiment the injured half of the egg remained in contact with the developing half.

Later experiments on other forms—the Sea-urchin's egg, for example, where the blastomeres can be completely separated—gave other results. A whole embryo developed from the isolated parts. Roux's conclusions were said to be overthrown. Then came an unexpected result. The blastomeres of the ctenophor may be com-

pletely separated, as perfectly as those of the echinoderm, but in the ctenophor the isolated blastomere develops into a half-embryo. Evidently, then, any new theory of development must explain how in one case it is possible for an isolated blastomere to develop into a half-embryo, and how in another case into a whole embryo. Perhaps the explanation is not far to seek, for it has been found that in one and the same egg the blastomere may under certain conditions give rise to a half-embryo, and under other conditions to a whole embryo of half size.

I might, had I time, cite many other experiments: those, for instance, in which a part of the unsegmented egg has been removed; Boveri's experiment in which a non-nucleated piece of an egg is entered by a single spermatozoön and an embryo forms; the experiments and observations of the direction of the nuclear spindle in the dividing cell; the experiments on the effects of different salts on development; the effects of light, heat and electricity on the egg or embryo, etc.

These experimental studies will serve as examples of the kind of work of the new embryology.

The two instances that I have already given—the effect of gravity of the egg, and the behavior of isolated blastomeres—teach us that the greatest precaution must be used before we can know whether a suggested mechanical explanation is really a true explanation. There is, I think, but one way in which we may hope to find out what forces or energies are at work during development, and whether these forces are the same forces known to the chemist and physicist. Only by means of well-planned experiments can we expect by isolation and recombination to discover the forces at work. Here, it seems to me, we find at least the real meaning and strength of developmental mechanics.

I admit freely that developmental mechanics is not a fortunate expression, but, nevertheless, Roux and his school have from the start encouraged experimental methods.

Perhaps it would be more appropriate to call the new work 'experimental physiology' of the embryo, using physiology in a wider sense than that usually given to it. For myself, I think our aim is reached if we use the term experimental embryology.

The history of science teaches us that by means of experiment chemistry and physics have made enormous progress; by means of experiment animal and plant physiology have become more exact, more profound studies than animal and plant morphology, and the department of bacteriology shows how rapidly and surely a study may advance by this method. Therefore, by means of experiment the student of the new embryology hopes to place the study of embryology on a more scientific basis.

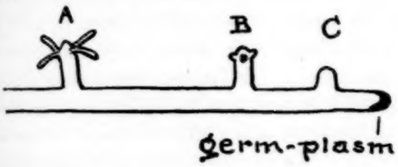
Morphogenesis. Dr. Chas. B. Davenport, Harvard University.

Morphogenesis may be defined as the study which attempts to explain the development of the form of the individual (ontogenesis) and of the race (phylogenesis).

Morphogenesis is a subdivision of general physiology, inasmuch as it deals with activities—processes, and, indeed, the largest, most complex biological processes, those by which the course of individual development is controlled and the direction of evolution is determined. Morphogenesis includes developmental mechanics in so far as that study attempts to explain the ontogenetic processes.

The scope of morphogenesis, embracing, as it does both ontogenetic and phylogenetic processes, is a broad one. Too broad, some may say who believe that there is no close relation between phylogeny and ontogeny; that ontogeny goes its way and phylogeny

goes its way and neither takes account of the other. Others do not share this view. They look upon the soma and the germ as very intimately bound together—associated in much the same way as the stolon and the hydranths of a hydroid are.



The germ plasm at the tip of the stolon gives rise at intervals to hydranths very much as the germ plasm of other animals gave rise at intervals to somas. In both cases the germ plasm is modifiable to a limited extent by such modifications of the soma as result from starvation, reduction in general vigor, or the secretion of specific substances affecting the germ plasm. In both cases developing soma and germ may be simultaneously modified by external agents, so that while the developing generation C is being changed, future generations, D, E, etc., are being potentially changed in the same fashion because in the germ plasm. For example, although I do not know that the experiment has been tried, a dense solution might produce a spindling soma C and a spindling stolon, so that even if the solution were diluted again a spindling soma D would rise. By other agencies we may modify the protoplasm at the tip of the stolon so that it will thenceforth tend to produce modified hydranths. Just as the formation of the stolon and hydranth are parts of one developmental process, so are phylogenesis and ontogenesis parts of one process. Every ontogenesis is dependent upon a preceding phylogenesis and every phylogenesis is dependent upon a preceding ontogenesis.

I have said that morphogenesis seeks to explain the development of the individual and the race. When have we explained development? We have explained any effect when we know its immediate causes—that is to say, the essential conditions under which the effect occurs. We seek, then, to know the essential conditions under which phylogenesis and ontogenesis occur.

What general methods must we employ to learn these conditions of development? There are two principal methods: one is the method of observation of the differences in development under known dissimilar conditions; the second method, more applicable and more certain, is the method of experiment.

I may illustrate the way in which simple comparative observation and observation with experiment throw light upon the processes of development. The simple observation that in the tunicate Doliolum the sexual buds, detaching themselves from the ventral stolon, crawl over the surface of the animal to the dorsal stolon to arrange themselves there in regular order might have taught us that one of the conditions directing individual development is response of the different parts of the developing individual to stimuli coming from other parts of the organism. On the other hand, the experiments of Driesch upon the gastrula of sea-urchins enforced the fact vividly, for he found that even after the mesenchyme cells had been hopelessly mixed up by shaking the gastrula they still migrated toward their destined place. So, too, the observation of the decline of the descendants of famous men might have led us to the law of regression toward mediocrity as a condition of phylogenesis just as Galton's experiments with sweet peas did. In the foregoing cases there is, however, a precision and decisiveness about the experimental method which marks it as one to be preferred where applicable. In addition to experiment, an allied method applicable especially to the study of variation is that

of statistics. As by experiment we make all causes similar except one and note the result, so in statistics we select results having at least one common cause and throw all together, believing that, from the doctrine of chances, all other causes will offset and annul each other. Thus we find, by comparing the mean in the selected group with the mean of the whole population, the effect of the particular cause used as a basis of selection.

So much for definitions and general methods. But I have been asked to suggest particular problems in morphogenesis and the methods of their solution. Of ontogenetic problems we have the question in how far is the development of the individual to be explained as a series of responses to the action of stimuli; not merely of stimuli external to the organism, but of part acting on part 'as in the marvelous automaton'—to use Aristotle's phrase. We get indirect evidence upon this matter in studying the capacity and laws of response in unicellular organisms; we get direct evidence by applying particular agents, such as light, heat and chemical substances, and noting their effect on development. Again, we have the question in how far the development of the individual is determined by wholly internal factors. To get an answer to this question one must mutilate the form and study the laws and limits of its restoration—regeneration, reparation, healing, development despite untoward conditions (as in dermoid cysts), and self-regulation (or accommodation) in disturbed ontogeny. In how far is the regeneration of organism comparable with that of crystals?

Next we come to a number of problems connected with both ontogeny and phylogeny. Such are the problems of adaptation. There is adaptiveness in those responses to stimuli that are met with in development—in tactisms, trophisms and differentiation. There is adaptiveness also in regeneration and self-regulation of the organism. These ontogenetic adaptations are often curiously dependent on the past history and habits of the species. Thus, Amœba dwells in dim light and is negatively phototactic, stems of plants which live in the dark turn from the sun, parts of an animal most apt to be lost are frequently those most capable of regeneration. Is it due to selection or is it an inherent quality of all protoplasms that they should respond thus advantageously? Or is this whole phenomenon of adaptation merely an ignis fatuus—this apparent shaping of means to ends only a necessary, mechanical relation? These questions can be answered by paying attention to cases of unadaptive response and unadaptive regeneration and regulation.

Finally, the strictly phylogenetic problems deserve far more attention than has yet been given them. Such are the questions concerning individual variation. It is well known that in some cases the measurements of an organ in the different individuals of a species group themselves about a mean value in accordance with the normal probability-of-error curve. In the case of species undergoing change, however, the curve is often very unsymmetrical or perhaps has several maxima. What is the precise meaning, in any case, of these abnormal curves? Again, how does the mode (or the most common measurement) vary with the habitat or geographical position of the varieties of the species? What is the significance of those large variations which we call sports and how do they differ in origin from individual variations? What sorts of variations in the body are correlated? What is the morphogenetic kinship of the various organs of the body? Then there are the questions dealing with inheritance: The laws of normal inheritance—Do the progeny of a particular cross inherit, on the average, equally from the

two parents in all cases—or is there such a thing as sexual or racial prepotency? Do sports show a prepotency in breeding? What are the limits of inheritance—to what extent and to what degree are modifications of the soma transmissible? What are the laws and limits of crossing—the capacity of hybridization; the abnormal distribution—the patchwork intermingling—of parental characters in the body of the adult hybrid? Next there are the questions, allied to those of crossing, respecting the reciprocal effect between scion and stock in grafting. In how far is there such an effect and what is its cause? How about the phenomena of telegony in animals and of xeny in plants? Finally, there are the momentous questions concerning the relative importance of selection, of sporting with segregation of the aberrant individuals, of crossing and hybridization, and of self-adaptation in the origin of species.

Now, these problems are comparatively untouched. Yet they are recognized as immensely important. The reason why they have not been worked upon is largely because they don't lend themselves to investigation in the laboratory. For the successful study of these problems one needs, indeed, not an ordinary laboratory, but a farm or an extensive zoological reserve with hothouses, breeding ponds, insectaries and vivaria of various sorts. With such means at his disposal a naturalist might hope, during a long series of years, to answer many of these fundamental phylogenetic questions.

CURRENT PROBLEMS IN PLANT MORPHOLOGY.

RELATIONSHIP BETWEEN PTERIDOPHYTES AND GYMNOSPERMS.

THE year 1897 will always remain a memorable one in the annals of plant morphology on account of the illuminating discoveries made by Ikeno * and Hirase † of spermatozoids in Cycas and Gingko, by Webber ‡ of spermatozoids in Zamia, by Belajeff § ‖ and Webber ¶ Sm ** of important new facts in spermatogenesis, and by Bower†† of new evidence bearing upon the homologies of spore-producing members.

These investigations, with others somewhat less notable, have already resulted in some important modifications of taxonomic sequence. Engler‡‡ divides the subdivision Gymnospermæ into two series—(*a*) those with functional spermatozoids, including here the Cycadaceæ, Gingkoaceæ and fossil Bennettitaceæ and Cordaitaceæ, each order having also the rank of a class, and (*b*) those with reduced spermatozoids (*Spermakerne*), including the classes Coniferæ and Gnetales. Thus the aberrant genus Gingko has been removed from the order Taxaceæ of the Coniferæ and made the type of a new order, which constitutes a

* Ikeno, S. Vorläufige Mittheilungen über die Spermatozoiden bei Cycas revoluta. *Bot. Centralb.* 69:1–3. Ja. 1897.

† Hirase, S. Untersuchungen über das Verhalten des Pollens von Gingko biloba. *Bot. Centralb.* 69:33–35. Ja. 1897.

‡ Webber, H. J. Peculiar Structures occurring in the Pollen tube of Zamia. *Bot. Gaz.* 23: 458. *note.* Je. 1897.

§ Belajeff, W. Ueber den Nebenkern in Spermatogenen Zellen und die Spermatogenese bei den Farnkraüten. *Ber. Deutsch. Bot. Gesellsch.* 15: 337–339. 27 Jl. 1897.

‖ Belajeff, W. Ueber die Spermatogenese bei den Schachtelhalmen. *Ber. Deutsch. Bot. Gesellsch.* 15:339–342. 27 Jl. 1897.

¶ Webber, H. J. The Development of the Antherozoids of Zamia. *Bot. Gaz.* 24:16–22. 31 Jl. 1897.

** Webber, H. J. Notes on the Fecundation of Zamia and the Pollen tube apparatus of Gingko. *Bot. Gaz.* 24:225–235. 30. O. 1897. (See also *Webber* ‡.)

†† Bower, F. O. Studies in the Morphology of Spore-producing members. The Marattiaceæ. Lond. 1897.

‡‡ Engler, A. Nachtrag zu Teil II.–IV. Pflanzenfam. 341. 1897.

class by itself coördinate with the Coniferæ as a whole. Again, Engler's great division of Embryophyta Siphonogama is seen to be unfortunately named, precisely as was pointed out by the writer* as long ago as 1892, for apparently Salvinia has the same right to be described as siphonogamous that Zamia has, and the real difference between seed plants and archegoniates lower than the Cycads appears to be one that lies rather within the sphere of ecology than in that of morphology; the seed coming into existence, perhaps, in more than one phylum through symbiotic relationships established between sporophytic and gametophytic plants of a species. In brief, the group of Spermatophyta, while ecologically, is not morphologically, homogeneous.

Van Tieghem,† whose interesting innovations in the taxonomic arrangement of higher plants seem to have attracted less attention than their various merits deserve, founds a new type of what he terms basigamous fecundation (*basigamie*), upon the investigations by Webber of Zamia. A useful criticism of Van Tieghem's general notions regarding flowering plant taxonomy, as set forth in various papers since 1894, ‡ § ‖ will be found in the work of Engler previously cited. It is interesting to note how the new light has been welcomed in both these taxonomic systems—Engler's, representing, upon the whole, the most modern development of conservative and slowly developed ideas, especially those of Eichler, and Van Tieghem's, representing, in a radical and novel manner, the influence of recent cytologic work on the embryo-sac and upon ovular development.

The importance of these discoveries upon spermatogenesis in particular have, however, much more than a formally taxonomic interest, for they open up in an inspiring way a number of cytological and morphological problems. They cast new light upon the vexed question of the sequences among Pteridophytes and make more certain the general acceptance of Bower's doctrines regarding the basal position of the Lycopodiaceæ rather than of the older view of Prantl, reaffirmed by Goebel, that Hymenophyllum-like ancestors connected the fernworts with the mossworts, or the view of Campbell, who sought in Ophioglossum the primitive type. For in the spermatogenesis of Zamia may be seen a recapitulation of spermatozoid improvement, and one learns how the biciliate form found in club mosses might, by the gradually increasing elongation of Webber's *blepharoplast*, or cilium-producing organ, be converted into the multiciliate form found in eusporangiate ferns and continued among higher ferns, in Cycads and in the Gingkoaceæ. Indubitably, then, the new investigations strengthen greatly the position that the biciliate-spermatozoid forming Lycopodinæ are, as a class, lower than the Filicinæ with their multiciliate spermatozoids. Precisely the same result has been reached by investigations on the sporophytic side, notably by Bower in the paper cited and in others.*†

Regarding the phylogeny of Pteridophytes

* MacMillan, C. Metaspermæ of the Minn. Vall. 25. 1892.

† Van Tieghem, Ph. Sur une nouvelle sorte de Basigamie. *Journ. de Bot.* 11: 323-326. 16 Oct. 1897.

‡ Van Tieghem, Ph. Acrogamie et Basigamie. *Jour. de Bot.* 9:465-469. 16 D. 1895.

§ Van Tieghem, Ph. Sur les Phanérogames sans grains, etc. *Comptes Rend.* 124: 590-595. 22 Mr. 1897.

‖ Van Tieghem, Ph. Classification nouvelle des Phanérogames. *Comptes Rend.* 124: 919-926. 3 My. 1897.

* Bower, F. O. Studies in the Morphology of Spore-producing Members. Equisetineæ and Lycopodineæ. Phil. Trans. Roy. Soc. Lond. 185 B. 473-572. 1894.

† Bower, F. O. Studies in the Morphology of Spore-producing Members. II. Ophioglossaceæ. London. 1896.

the following points may be regarded as settled:

1. *Phylloglossum drummondii* is the simplest known living Pteridophyte.

2. The fertile spike of Ophioglossum is derived by sterilization in transverse planes of a bilocular Tmesipteris-like sporangium.

3. *Tmesipteris tannensis* is, of known forms, the club moss nearest to the Filicinæ.

4. The sporangia-bearing leaves of Marattiaceæ and Leptosporangiate ferns homologize with the fertile spikes alone of Ophioglossum leaves.

5. In the sense in which sporophylls occur in Lycopodinæ they also occur in ferns among the Ophioglossaceæ, but in Marattiaceæ only the stipules remain to represent the old sporophyll lamina of archetypal ferns. In Leptosporangiate ferns even the stipular vestige has disappeared.

6. The Isœtaceæ, Salviniaceæ and Marsileaceæ are terminal groups.

7. Either seed-producing plants are of polyphyletic origin or the multiciliate type of spermatozoid has been developed in more than one branch of archegoniate plants.

Concerning the latter point it is proper to observe that most students of phylogeny have looked for the archetype of the Coniferæ in the vicinity of Selaginella. It will be noted that all genera of seed-producing plants known to produce spermatozoids do not produce biciliate, but multiciliate, spermatozoids. The embryogeny, however, of Lycopodinæ, notably of Selaginella, is much more similar to that of seed-producing plants than is the embryogeny of such ferns as Isœtes. With Isoetes and the Marattiaceæ the Cycads, however, show some striking points of similarity, and it may be that this group of seed-plants is allied rather with Filicinæ than with Lycopodinæ. If it be accepted that the prevalent pinnation of fern leaves is really of very profound significance and indicates the presence among their ancestral types of a Tmesipteris-like form with bilocular bilateral sporangium, capable of development into the fertile spike of Ophioglossum, it may also be held as probable that the pinnation of carpellary leaves of Cycas has a similar profound significance. On the other hand, the strobiloid features of the Zamiæ may also be fundamental and a Selaginella-like ancestor may, therefore, be proposed. In this case the multiciliate sperm of Gymnospermæ would be held to have an independent origin as compared with the multiciliate sperm of Filicinæ and Equisetinæ.

Of Equisetum, indeed, the archetype among living club-mosses would seem to be most closely reproduced in Psilotum with its radially trilocular synangia, and the difference between radial and bilateral synangia may be as important as indicated by Celakowski.* According to this student of phylogeny the radial type of synangium is the most ancient and is perpetuated in Gymnosperms as well as in Equisetinæ. There are, however, three types of synangia derived by sterilization of simple Lycopodium-like sporangia. These are as follows:

1. The radial type. Exemplified in Psilotum, Equisetum, Taxus.

2. The bilateral type. Exemplified in Tmesipteris and leading to the fertile spike of Ophioglossum and to the 'sporangiophyll' of Marattiaceæ and Leptosporangiate ferns and possibly to the carpels and stamens of Cycas.

3. The reticular type. Exemplified in Isoetes.

In Cycadaceæ the sporangial type is, from the pinnation of carpels, originally not trabecular, but bilateral, indicating either a Tmesipteris-like ancestor or an independent bilateral modification of the Selaginella

* Celakowski, L. J. Nachtrag zu meiner Schrift über die Gymnospermen. Engl. *Bot. Jhrb.* 24:202–231. 17 Ag. 1897.

type of unilocular sporangium. It is therefore impracticable to connect Cycas with Isoetes, on account of the sporangial structure, and either an independent development of heterospory must be assumed for the Cycadaceæ or they must be connected with the Coniferæ and traced back to Selaginella. That is, the evidence on the whole points to an independent deve opment in Cycadaceæ, and also probably in Gingkoaceæ, of multiciliate spermatozoids. This conclusion is borne out by the marked peculiarities of Cycad and Gingko spermatozoids as described by Ikeno, Hirase and Webber, although important resemblances between the development of the spermatozoid in Zamia, as described by Webber, and in Equisetum, as described by Belajeff, must be conceded.

The researches most needed at present are upon the genesis of the sperm-nucleus in Coniferæ and Gnetales to discover whether bodies which might be regarded as reduced blepharoplasts are present, and if so whether they indicate a multiciliate or biciliate spermatozoid in primitive Taxaceæ.

In general, it may be said that the polyphyletic theory of the origin of the gymnosperms is strengthened by the new researches, but it remains more difficult than before to include Isoetes-like forms among the probable ancestors of seed-plants. In addition, the very considerable differences between gymnospermous and angiospermous seeds arising from the wide variance in endosperm formation, together with the singular inversion (?) of the female plant in VanTieghem's Basigameæ and the suppression of the ovule in his Inovuleæ and of the nucellus in the Innucelleæ, together justify the view that the Spermatophyta is not a homogeneous group, but is purely ecological, comprising groups of widely different phylogeny, but, in general, similar adaptations arising under what I have previously termed symbiotic alternation of generations.

CONWAY MACMILLAN.

PALEONTOLOGICAL NOTES.

AMONG the recent papers of Mr. A. Smith Woodward, of the British Museum, are several matters of general interest in paleontology. Referring to Professor Marsh's discussion at Ipswich of the 'Jurassic Age of the Wealden Vertebrate Fauna,' Mr. Woodward has listed the Wealden fishes very carefully, and concludes as follows:*

"The result is, therefore, that all the known English Wealden fishes are survivors of typically Jurassic genera, except *Neorhombolepis* and *Cœlodus*, and these are their little-modified representatives. None but *Belonostomus* appear to range throughout the Cretaceous. In fact, the Wealden estuary seems to have been the last refuge of the Jurassic marine fish fauna in this part of the world, not invaded even by stragglers from the dominant race of higher fishes which characterized all the seas of the Cretaceous period. The Wealden river drained a land where a typically Jurassic flora flourished; the only two known Mammalian teeth from the Wealden resemble those of a Purbeckian genus, and now it is clear that the fishes agree both with these and the reptiles in their alliance with the life of the Jurassic era."

The second note relates to the occurrence of a gigantic Pterodactyl in the Cretaceous of Bahia, Brazil, and concludes with the following note:†

"Not being able to determine the genus of the Brazilian Cretaceous Pterodactyl, it is equally impossible to estimate the size of the skull or the animal itself from a single bone. There is too much variation from the proportions of the snout and the relative dimensions of the head among Pterodactyls to admit of any such induction. To judge by Marsh's figure of the skull of *Pteranodon*, however, the Brazilian form must have even exceeded in size the gigantic species of this North American genus, of which the head sometimes attains a length of four feet."

* 'On the Affinities of the English Wealden Fish-Fauna.' *Geol. Mag.*, Vol. III., No. 380, p. 69.

† 'On the Quadrate Bone of a Gigantic Pterodactyl,' etc. *Annals and Magazine of Natural History*, Ser. 6, Vol. XVII., 1896.

In a third paper appears the translation of Ameghino's recent summary of his researches upon the geology and paleontology of Argentina,* followed by a critical review of the same by the writer, who has recently examined the Argentine collections. He does not accept the evidence of the very great age assigned to the 'Pyrotherium' and overlying Beds and urges "that Señor Ameghino should show quite clearly why Pyrotherium cannot be a close ally of the large Australian Diprotodonts. It certainly differs from the Proboscidia in some of the most fundamental characters." In the Red Sandstones with Dinosaurian remains Dr. Santiago Roth has recently brought back a fine collection of small reptilia.† One of these is a typical and apparently fully-evolved snake, which the author had no time to study in detail. The others are small crocodilia, particularly interesting because they are typical Mesosuchia with the characteristic palate and amphicœlous vertebral centra. They seem to be most closely related to the small Purbeckian *Theriosuchus* and its allies, differing, among other features, in their more highly specialized dentition, and referable to a new genus, which the author names *Notosuchus*.

Another important note‡ relates to a new specimen of *Sterosternum tumidum* from the State of San Paulo, Brazil, interesting as showing for the first time the general proportions of the trunk and tail of this strange extinct reptile. Stereosternum was originally described by Professor Cope in 1886, and in the same year Dr. Baur made it a type of the new Reptilian order termed Proganosauria. It is now evident, according to Woodward, that the animal is related in some undetermined way to the ancestry of the Plesiosauria. The head is of an elongate triangular form, but much shorter than the neck. The tail possesses not less than sixty vertebræ, of which the foremost seven bear robust transverse processes. As a whole the tail is thus somewhat more than twice as long as the trunk, occupying slightly less than three-fifths of the length of the entire animal. Dr. Derby has also obtained a typical Labyrinthodont tooth from the Silicious Limestones at Conchas. "In fact," Mr. Woodward concludes, "evidence is gradually accumulating to render it still more certain that the Karoo Series of South Africa is well represented by homotaxial deposits in the south of Brazil and in parts of the Argentine Republic."

A new specimen of Ceraterpeton* from the Coal Measures of Castlecomer, Kilkenny, Ireland, is the second example from the typical locality of Huxley's original description thirty years ago. This specimen found by Mr. Robertson is of special importance in making known for the first time many characters of the scapular arch and limbs. It now appears that the amended definition based by Fritsch upon specimens from the lower Permian of Bohemia does not apply to the genus with which Huxley was dealing when he originally proposed the name. The generic name *Scincosaurus* originally applied by Fritsch to his Bohemian specimens ought thus to stand. The skull from the Coal Measures of Ohio described by Cope under the name *C. lennicorne* seems, however, to be correctly placed here; but of this animal the trunk still remains unknown.

H. F. O.

* 'Geology and Paleontology of Argentina.' *Geological Magazine*, Vol. IV., No. 391, p. 4, 1897.

† Ceraterpeton Galvani, Huxley. *Geological Magazine*, July, 1897.

‡ Stereosternum from Brazil. *Geological Magazine*, March and April, 1897.

CURRENT NOTES ON ANTHROPOLOGY.

DEFORMED SKULLS FROM GUATEMALA.

AT a recent meeting of the Berlin Anthropological Society Professor Virchow ex-

*Ceraterpeton Galvani, Huxley. *Geological Magazine*, July, 1897.

hibited and described several skulls from ancient graves in the Kekchi district of Guatemala, brought by Mr. Dieseldorf. They were deformed to an unusual degree, to an extent, indeed, not equalled elsewhere in America. The method of deformation was like that of the Natchez Indians, the forehead flattened and pushed back and upward. Just this deformity is seen on many of the Mayan art works, and instead of being caricatures such are regarded by Professor Virchow as actual imitations of this custom of malformation.

They were very fragile, indicating a high antiquity, and the objects associated in the tombs whence they were derived showed them to be pre-Columbian in age. It will be remembered that from these tumuli Mr. Dieseldorf obtained some of the most artistic pottery products known in America.

NATIVE AMERICAN STRINGED INSTRUMENTS.

This subject is again discussed in a brief article by Professor Otis T. Mason in the *American Anthropologist* for November last. His conclusion is as follows: "After looking over the musical collection of the United States National Museum and such literature as has been collected by the Bureau of American Ethnology I have come to the conclusion that stringed musical instruments were not known to any of the aborigines of the western hemisphere before Columbus."

While the opinion of one so competent as Professor Mason on this subject demands the utmost respect, some of the examples which I quoted in the *American Antiquarian* (January, 1897) are not considered by him, and seem to present a moderate amount of evidence that the musical string was not wholly unknown to the American race by independent discovery.

D. G. Brinton.

University of Pennsylvania.

NOTES ON INORGANIC CHEMISTRY.

In a recent *Comptes Rendus* Moissan calls attention to the fact that calcium carbid is a powerful reducing agent, and hence, when in a fused condition at a high temperature, can furnish, by double decomposition, a number of new compounds. When acting on metallic oxids the metal may be obtained in a free state, or if it is capable of uniting with carbon a carbid is formed. By this reaction Moissan has prepared crystallized carbids of aluminum, manganese, chromium, molybdenum, silicon, etc.

According to the *Journal de Pharmacie et de Chimie*, Dutremblay and Lugan expect to make a commercial success of the manufacture of oxygen by the manganate method. The process consists of decomposing manganates of the alkalies by steam at 500°, and then regenerating the manganates by heating in a current of dry air. This process was used by Tessié du Motay some thirty years ago, but afterward abandoned, owing to the caking of the charge and evaporation of the soda, there being great danger of explosions. It is hoped these dangers have been now overcome, and that the process will be a success.

A careful study of the valence of glucinum by Arthur Rosenheim and Paul Woge appears in the *Zeitschrift für Anorganische Chemie*. A considerable number of double oxalates and tartrates of glucinum and alkalies was prepared, and in all glucinum shows analogy with the bivalent and never with the trivalent metals. The same is true in its molybdate and in the double glucinum alkali sulfites. A more exact proof of its bivalence was shown by the determination of the molecular weight of the chlorid by the boiling-point method, pyridin being used as a solvent. The molecular weight corresponded to the formula $GlCl_2$. The conclusion of the authors is that glucinum is bivalent, and is

rightly placed in the second group of the periodic table, thus confirming the generally accepted views of chemists.

J. L. H.

SCIENTIFIC NOTES AND NEWS.

INTERNATIONAL CONGRESS OF ZOOLOGY.

AT the meeting of the International Congress of Zoology at Leyden, in 1895, it was agreed that the Fourth Congress should be held in Great Britain, and that the President should be Sir William Flower, K.C.B., F.R.S. As we have already announced, the Permanent Committee of the Congress accepted an invitation to assemble at Cambridge in August, 1898. Sir W. Flower was compelled to resign on account of ill health and Sir John Lubbock was unanimously selected in his place.

The seat of an ancient University, which counts among its alumni distinguished zoologists from the days of Ray and Willughby to those of Charles Darwin and Francis Balfour, seems to offer a peculiarly fit meeting-place for the Congress on its first visit to the British Islands, and the Reception Committee, including the present representatives of zoological science in Cambridge, offer a cordial welcome to their brethren at home and abroad.

The officers of the Congress are: *President*, Right Hon. Sir John Lubbock, D.C.L., F.R.S.; *Vice-Presidents*, The Vice-Chancellor of the University of Cambridge, Mr. W. T. Blanford, LL.D., F.R.S., Sir W. H. Flower, K.C.B., D.C.L., F.R.S., President of the Linnean Society (Dr. A. Günther), Professor E. Ray Lankester, LL.D., F.R.S., Professor A. Newton, F.R.S., Mr. P. L. Sclater, F.R.S., President of the Entomological Society (Mr. R. Trimen), Sir William Turner, F.R.S., Lord Walsingham, LL.D., F.R.S.; *Treasurers*, Professor S. J. Hickson, F.R.S., Mr. P. L. Sclater; *Secretaries*, Professor F. Jeffrey Bell, M.A., Mr. G. C. Bourne, M.A., Mr. A. Sedgwick, M.A., F.R.S., and a large general committee.

The Executive Committee, appointed by the General Committee at their meeting on November 4th, have now made the necessary preliminary arrangements for the holding of the Congress in August next. The Reception Committee hope to avail themselves largely of the facilities offered by the several colleges of Cambridge for the accommodation and entertainment of their visitors, while there is assurance that the more suitable of the public buildings of the University will also be placed at their disposal for the same purposes.

The International Congress of Physiology is to meet in Cambridge concurrently with that of Zoology, and certain arrangements will be made in common, though there is no intention of uniting the two Congresses, each of which will retain its distinct organization.

The Secretary has issued an appeal for funds that will be necessary to carry out the purposes of the Congress. Some members of the Executive Committee and others have already intimated their intention to make donations, and a list of these will be found below. Cheques should be sent to P. L. Sclater, Esq., F.R.S., or Professor Hickson, F.R.S., the Hon. Treasurers, at 3 Hanover Square, London, W.

DONATIONS ALREADY PROMISED.

	£.	s.	d.
Right Hon. Sir John Lubbock, Bart, M.P.	50	0	0
Hon. Walter Rothschild	50	0	0
A. Peckover, Esq	50	0	0
Sir William Flower, K.C.B., D.C.L., F.R.S.	25	0	0
The Lord Powerscourt	5	0	0
The Lord Walsingham, F.R.S	5	0	0
P. L. Sclater, Esq., F.R.S	5	0	0
Howard Saunders, Esq	3	3	0
R. Trimen, Esq	3	3	0

GENERAL.

HON. CARROLL D. WRIGHT, United States Commissioner of Labor, has received a cable dispatch announcing his election as a member of the Institute of France. He has also been elected an honorary member of the Imperial Academy of Science of Russia.

WE learn from the Philadelphia *Medical Journal* that a portrait of the late Dr. Theodore G. Wormley, professor of chemistry and toxicology in the medical department of the University of Pennsylvania, has been subscribed for by the students of the medical, veterinary and dental departments of the University, and by members of the faculty. It is to be formally presented to the Board of Trustees at the next commencement of the University.

THE subscriptions for the American University Table have been received from Brown University and from the Marine Biological Laboratory through Professor H. C. Bumpus; also from the American Society of Naturalists through the Treasurer, Professor Smith, amounting altogether to $250. They have been forwarded to Dr. Anton Dohrn.

ACCORDING to the *London Times* Mrs. Louisa C. Tyndall has written the following letter to Sir James Crichton-Browne, the Treasurer of the Royal Institution of Great Britain:

JANUARY, 1898.

DEAR SIR JAMES: As an expression of his attachment to the Institution, with which he was so long connected, and of his sympathy with its objects, my dear husband desired me (at such time as should be most convenient to myself) to present in his name to the Royal Institution £1,000, to be disposed of as the board of managers may see fit for the promotion of science.

I have now the pleasure of remitting to you this sum.

Yours faithfully,
LOUISA C. TYNDALL.

Sir James Crichton-Browne, in acknowledging the communication, says:

DEAR MRS. TYNDALL: I have to acknowledge your letter enclosing a crossed cheque of the value of £1,000. This generous donation to the funds of the Royal Institution, given by your late husband's expressed wish, will be notified to the managers and to the members generally at their next meeting, when a formal acknowledgment of their grateful appreciation of it will be communicated to you. Meanwhile, I trust you will allow me to express my own sense of the munificence of the gift, and of the simple and touching terms in which it has been conveyed. The managers would, I am sure, desire to be guided by any wish of yours as to the application of the gift; but, in the absence of any explicit directions, they will, I have no doubt, employ it in the promotion of that original scientific research in which your husband's vivid and penetrating intellect delighted to exercise itself. Revered as your late husband's memory is, and ever must be, in the Royal Institution, this posthumous mark of his solicitude for its welfare will, if possible, deepen the affectionate esteem in which he is held. There is not, I regret to say, in the Royal Institution any worthy presentment of the late Professor Tyndall. You have, I believe, an admirable bust of him by Woolner, and I should be glad to know if you would feel disposed to afford facilities for having a replication of that made for the Royal Institution.

WE regret to announce the deaths of Arthur Kammermann, astronomer, at Geneva on the 15th of December, at the age of 36 years, and of Dr. Oscar Stumpe, astronomer, at Berlin, aged 35 years.

THE thirtieth annual meeting of the Davenport Academy of Natural Sciences was held January 5, 1898. At this meeting the following honorary members were elected: Professor Henry S. Pritchell, Supt. U. S. Coast and Geodetic Survey, Washington, D. C.; Professor Robert Etheridge, South Kensington Museum, England; Dr. B. E. Fernow, Chief of the Division of Forestry, Washington, D. C.; Dr. John S. Billings, Director of the Consolidated Libraries of New York.

PROFESSOR LUCIEN M. UNDERWOOD, of Columbia University, lectured before the Philadelphia Academy of Natural Sciences on January 8th. The subject of the lecture was 'Our Native Fungi and How to Study Them.' A paper on 'The Law of Regression in Plants' was read by Professor J. C. Arthur before a recent meeting of the Minnesota Academy of Science at Minneapolis.

MR. B. E. FERNOW, Chief of the Division of Forestry, has been called to Hawaii to make a reconnoissance and to report concerning desirable forestry legislation.

BY the will of former Chief Justice John Scott, his estate, amounting to about $2,000,000, is to be held in trust for the benefit of his heirs until their death, when it is to go to the city of Bloomington for the foundation of a hospital.

DR. HERBERT HAVILAND FIELD writes from Zurich that the Zoological Bibliography has not yet received adequate support in the way of subscriptions in this country and is being conducted at a considerable personal loss. Save by Cornell University, there are no subscriptions either to the Physiological or the Anatomical Cards in the States of New York, Connecticut and New Jersey. The Sandwich Islands are better off, since Honolulu has three full subscriptions, besides several parts. This lack of support is

partly due to the fact that this valuable Bibliography, which is offered in either card catalogue or in sheet form with brief abstracts of the papers catalogued, has not been sufficiently advertised in this country. Although an international undertaking, it is largely due to the enterprise and scientific spirit of Dr. Field, and deserves the warm support of every institution in this country.

IT is stated in the *Bulletin* of the New York Public Library that the total number of periodicals and transactions of societies to which the library is subscribing, for the year 1898, is 2,502. Of these 483 are American, 497 British, 595 French, 660 German, 125 Italian, 36 Scandinavian, 27 Belgian, 16 Dutch and 12 Russian. During the calendar year ending December 31, 1897, the total number of volumes received by purchase was 16,098, and by gift, 10,128, making a total of 26,226. The total number of volumes catalogued and accessioned during the same period was 29,792. The number of pamphlets actually received during the year, by purchase, was 10,350, by gift, 40,247, and the total number catalogued and accessioned was 15,274. The total number of cards written during the year was 156,925. In addition to this, 15,404 slips from the printer were written, and for each of these slips five printed cards were obtained. The total number of cards in the Index Catalogue, which was open to readers, on the 31st of December, 1897, at the Astor Branch was about 80,000, at the Lenox Branch it was 27,800. The total number of readers during the year was 103,384, and the number of volumes called for by readers' slips, outside of those taken from the free reference shelves, was 304,466.

IN accordance with the directions of the Pennsylvania Fish Protective Association, the Executive Committee of that body has drafted and forwarded the following letter to President McKinley with respect to the appointment of a United States Fish Commissioner.

PHILADELPHIA, January 13, 1898.

To the President of the United States: The public press has recently announced a contemplated change in the office of the United States Commissioner of Fish and Fisheries. The Pennsylvania Fish Protective Association, fully recognizing the usefulness and high state of efficiency to which the work of this department of the government has been brought, would respectfully ask that, in making any appointment, due regard should be had to a compliance with the provisions of the statute providing for the proper qualifications of such Commissioner. We are, very respectfully,

E. HAGERT, *President.*
M. G. SELLERS, *Secretary.*

THE Cairo correspondent of the London *Times* writes: "The Egyptian government have abolished the important Fisheries Administration of Damietta and Lake Menzaleh, an antiquated institution under Levantine and native management, in which serious abuses had been discovered. In place of the former mode of collecting the revenue, by a duty on the fish caught, an annual tax is imposed on the fishing boats, and the alacrity with which this tax has been paid would indicate that the change is welcomed by the fishermen, though grave doubts are expressed lest unlimited license to fish uncontrolled by the teaching of science may result in depletion of the fishing grounds of Menzaleh, which comprise an area of 60,000 acres and are the resort of large shoals of salt-water fish. The new measure has involved the dismissal of Dr. J. C. Mitchell, scientific expert to the administration during 18 months and previously professor of zoology at the Ghizeh Agricultural College, who holds very complimentary testimonials from the Ministries of Finance and Public Instruction, also from her Britannic Majesty's Agent. It is to be regretted that government, in the present necessity for economy, cannot utilize his scientific attainments and fluent knowledge of Arabic in some other department. His abrupt dismissal after six years of good service has created an unpleasant feeling amongst the other officials who, like him, have accepted offers made by government, in the reasonable expectation that they were entering a permanent service."

THE Council of the Société d'Acclimatation has decided to issue, in addition to the Bulletin of the Society, a monthly journal which it is hoped will become the medium of communication between those interested in the objects of the Society. Great weight will be laid on the department of discussion, and the Secretary will

be glad to publish questions of a scientific or practical nature, the answers to which would not easily be found in existing publications. The journal will also contain announcements of the meetings of the Society.

THE Agricultural Department has issued a Farmer's Bulletin on the subject of forestry for farmers, by Mr. B. E. Fernow, Chief of the Division of Forestry. It contains articles in popular language regarding the growth of trees, the planting of forests, treatment of the wood lots, the cultivation of the wood crop, influence of trees, etc. The publication is a reprint from the year books of the Department for 1894 and 1895, and is issued in the present form because of the large demand for the information contained.

THE *Botanical Gazette* states that the collection of plants and literature made by the late Professor L. N. Johnson, of Ann Arbor, are offered for sale. Professor Johnson was especially interested in algæ, particularly the desmids, and has published a number of papers concerning them.

FROM the same source we learn that the complete herbarium of the late Mr. M. S. Bebb has been purchased by the Field Columbian Museum of Chicago, as also his letters, manuscripts, sketches, drawings, etc. The material of Mr. Bebb's own collecting was always known for its perfect preservation, but aside from the large general collection the special value of this purchase is to be found in the unique collection of salix material.

AT a meeting of the Fellows of the Royal Botanic Society of London held in the Gardens at Regents' Park on January 8th the Chairman, Major Cotton, congratulated the Fellows on the position at present occupied by the Society compared with its position a year ago. At that time the lease of the Gardens was about to expire, and there was an accumulated debt of some thousands of pounds. The debt, with the coöperation of the Council and of some of the leading Fellows of the Society, has since been paid and a new lease for the *maximum* term of 31 years has been promised by the Commissioners of Woods and Forests. The Gardens were opened for study to the students of the medical schools, and with the aid of the London County Council a school of practical gardening has been established. The Council of the British Astronomical Association were taking steps to erect and equip an observatory in the Gardens. Reference also was made to the great increase in the number of Fellows elected in 1897, there having been more than eighty above the average number of the last ten years. The receipt of a large number of donations to the library and museum was recorded, and a vote of thanks to the donors having been passed the meeting terminated.

AT a meeting of the Institution of Electrical Engineers, of London, on January 13th, Mr. J. W. Swan, F.R.S., the newly elected President, delivered his inaugural address, which took the form of a general review of the rise and progress of electro-chemical industries.

THE first general meeting of the Childhood Society of Great Britain, which was founded in November, 1896, was held in London on January 12th, Sir Douglas Galton, Chairman of the Society, presiding. It was reported that two courses of instructive lectures on the observation, study and training of children had been given. The Committee of the International Congress of Hygiene and Demography, appointed in 1891, having completed their work and issued a full report on 'the scientific study of the mental and physical conditions of childhood, with particular reference to children of defective constitution, and with recommendations as to education and training, based on the examination of 100,000 children,' handed over to the Society the balance of their funds in hand ; also the published copies of their report, and all records of preceding work, which formed a most valuable basis of future research now in the possession of the Society. Dr. F. Warner stated that the Society had now records of 1,120 children who appeared to require special care and training, and a report had been prepared showing the grounds upon which the opinion that special care and training were necessary was formed. At the close of the meeting Sir Douglas Galton delivered the opening lecture of the session on 'Measures to be taken for the Care of the Feeble-minded.'

THE last issue of the *Monthly Weather Re-*

view contains an abridgment of an account given in *Das Wetter* of the celebration of the semicentennial of the Royal Prussian Meteorological Institution. The jubilee festivities were divided into three parts: An address in Memorial Hall by the Director of the Institution, a visit of inspection to the Magnetic and Meteorological Observatories of the Institute, and a banquet in the hall of the Palace Hotel in Berlin. In his presidential address Professor von Bezold sketched the activity of the Institution during the whole period of its existence, showing the important part it had taken in the progress of science. The first Director, Mahlmann, held that office only a short time and was succeeded by Heinrich Wilhelm Dove, who, without controversy, elevated this meteorological institute to the highest position among all similar establishments throughout the world at that time. In the year 1885 the Institute was greatly enlarged and adapted to its new problems by the addition of the appropriate men of science. At the present time there are 188 stations of the higher class, 1,336 thunderstorm stations and 1,844 rainfall stations; scientific balloon ascensions on a larger scale than have hitherto been made also contribute material of the highest value for the study of the physics of the atmosphere. This material is reduced, analyzed and discussed at the Central Institute in Berlin; the distribution of meteorological knowledge is provided for by instruction at the University, given by members of the staff; the experimental investigations are conducted at the Meteorological and Magnetic Observatory at Potsdam. This latter institution, in connection with the astrophysical and geodetic institutions in the same locality at Potsdam, constitute altogether a microcosmos located, as it were, at a definite point on a line extending from the center of the earth outward to the stars. At the close of the address the great golden medal in science was presented to the Director of the Institute, von Bezold: the Order of the Crown (3d class) was given to Hellman, as Chief of the First General Division of the Central Institute; the Order of the Red Eagle (4th class) was bestowed upon Sprung, Chief of the Third or Instrumental Division and Director of the Meteorological Observatory; the Order of the Crown (2d class) was given to Vogel, Director of the Astrophysical Observatory. Professor Gruhn, of Meldorf; Professor Mohl, of Cassel; Professor Paszotta, of Konitz; the publisher, Alexander Faber, of Magdeburg, and, finally, Friedrich Treitsche, as proprietor of the Mountain Observatory, on Inselsburg, near Erfurt, received the Order of the Red Eagle (4th class).

THE number of applications for patents in Great Britain during 1897 shows an increase of 742 over the previous year and of 5,871 over those received during 1895. The number of patents applied for is not in itself a reliable index of the number of patents that may be issued. In the year 1896 of 30,194, 13,360 were completed, the rest being allowed to lapse after the nine months' protection. The inventions comprise every class of manufacture, but principally engineering.

THE *British Medical Journal* reports that Mr. Jonathan Hutchinson, F.R.S., intends to found a museum in his native town of Selby, in Yorkshire. Mr. Hutchinson has already established an educational museum at Haslemere, near which he has a residence, and here he has already a number of objects to spare, so that he hopes to be able to stock the new museum at Selby very rapidly. The new museum also is intended to be strictly educational, that is to say, it will contain objects calculated to convey knowledge to the less instructed, but at the same time it will not be confined to any particular subjects.

WE learn from the New York *Tribune* that the government at Ottawa has just instituted a change of policy regarding timber regulations applicable to the Northwest and Manitoba. Timber reserves will be maintained. With this object in view, the heavier timber belts will be withdrawn from settlement, and the young trees be preserved to provide a growth for the future. Guardians will be appointed to protect the reserves, particularly in the Turtle and Moose Mountain regions. Fires will be prevented, as far as possible, and the settlers will be restrained from cutting young trees. At the last session of Parliament a fund was voted for the institution of an efficient fire guard, and that will speedily be formed. At the summit

of the reserves mentioned are numerous lakes, and these are to be connected with wide roads, which, when completed, will, it is thought, form an effective obstacle to the progress of conflagrations.

UNIVERSITY AND EDUCATIONAL NEWS.

AT the semi-annual meeting of the Board of Trustees of Beloit College it was announced that the College had received a gift of $25,000 for the endowment of the chair of chemistry, now occupied by Professor E. G. Smith. The donor wishes to remain anonymous. It was also reported that the sum of $70,000 had been raised toward the $100,000 necessary to secure Dr. Pearson's gift of $50,000.

MRS. DANIEL C. EATON has recently given $2,000, the income from which is to be devoted to a scholarship open to competition by the graduate students of Yale University.

THE following assistants have been appointed in the Sheffield Scientific School of Yale University: C. B. Rice, in physics; W. G. Van Name, in biology: C. H. Warren and W. M. Bradley, in chemistry, and G. L. Bunnell, in zoology.

THE registration of students at the University of Pennsylvania for the year 1897–1898 is 2,834, an increase of 23 over the previous year. A decrease of 38 in the medical school is due to the raising of the requirements for admission. The officers of instruction number 258.

REPRESENTATIVE HAYES, of Lowell, has introduced into the Massachusetts House of Representatives a bill for a State appropriation of $100,000 to the Lowell Textile School, one-half of the amount to be paid in 1898 and 1900. At the same session Representative Dubuque, of Fall River, introduced a bill for an appropriation of $100,000 for the establishment of a school in that city upon the same lines as the one in Lowell.

DISCUSSION AND CORRESPONDENCE.

'WILD NEIGHBORS.'

EDITOR OF SCIENCE: A man who has been making books as long as I rarely 'talks back' to the critics. I never did so but once, and that was to rebut misstatements likely to injure the value of my property. For the same reason I beg leave to reply to your recent notice of my book 'Wild Neighbors' (The Macmillan Co., 1897), first thanking you for such commendation as is given.

Alluding to the fact that in order to round the biographies of the various animals treated, and make them interesting, I drew upon the writings of several 'well-known' naturalists, the reviewer so states this matter as to imply that the whole book is nothing but a mosaic of quotations, 'direct *and indirect*' (oh, fie!), and later frankly says that it 'offers nothing in the way of new and original matter.' It would be possible to produce an interesting and even valuable book in that way; but, if by the latter phrases quoted above it is meant that the book contains nothing of my own observation, I must protest. The chapter on Gray Squirrels distinctly states that it is wholly personal experience, and I have certainly seen on several occasions each of the other mammals described. As I did not write the book to laud myself, but to set the subject well before the reader, it did not occur to me invariably to put in the big I, yet I have not yet heard any complaint as to stolen goods.

Your reviewer alleges that 'many misleading statements are made,' and in support of this makes a very erroneous one himself. "The reader is told," he says, "that the Eastern Chipmunk (*Tamias striatus*) is now conceded to be the only species ranging between the Atlantic and Pacific coasts, while in reality some twenty-two species and twelve subspecies are now recognized in the United States." The reader is not told (by me) anything of the sort. He *is* told that the early naturalists, lacking large numbers of specimens, made several distinct species, so-called, of what are now conceded to be only geographical varieties of the single species *Tamias striatus*. There is a sort of sneer in the reviewer's next remark: "Young opossums are said to go about clinging to their mother's tails soon after they are born." That is not altogether a fair way of putting my account of it; but—don't they 'sometimes'? Credible persons say they do—Flower and Lydekker, for instance. Then the reviewer asserts that my 'nomenclature is out of date, a *large proportion* of the

generic and specific names differing from those in present use.' This, if true, would make me feel worse had I written a technical treatise instead of an untechnical one; but I should be thankful for a count of examples justifying this broad condemnation. All my names in classification (mainly relegated to the Index) are certainly as modern as the latest editions of Flower's 'Mammals' and Newton's 'Dictionary of Birds,' and are such as Dr. Elliott Coues and Dr. Theodore Gill thought proper for the Century and Standard Dictionaries. If they conform to these standard books of reference, and are rightly applied, I can safely say that if I had known (as possibly I did) of trinomial or other novelties of nomenclature more recently introduced by some specialist I would not have used them in a book for popular educational reading. The only reason for printing a technical name at all in such a book is that it may assist the reader in identifying the creature for further study elsewhere—an object that would be defeated unless a well-known term were quoted. If the reviewer had commented in this spirit upon this point, criticising the paucity, or what he considers the antiquated character, of such nomenclature as he found, I should never have alluded to it; but as he seems to bring it forward only as another symptom of general worthlessness, I deny the deficiency he reports.

A reviewer may combat my opinions or arguments or literary expression, and I shall be patient; or, if he can find real errors as to fact (as this one and others have done in noting a regrettable slip about the nuthatch) I shall be sorry and docile; but when he misstates my language, and resorts to innuendo instead of criticism, I shall resent it. First of all, a reviewer ought to try to understand the *purpose* of the book before him.

Ernest Ingersoll.

New York, January 8, 1898.

In replying briefly to the above, let me begin by quoting verbatim what Mr. Ingersoll does say about the Eastern Chipmunk: "The chipmunk (*Tamias striatus*) * * * *, whose color and stripes exhibited so many varieties between the Atlantic and Pacific coasts that early naturalists having insufficient specimens described confidently as several species what is now conceded to be only one." But, as a matter of fact, the Eastern Chipmunk (*Tamias striatus* and varieties) does not range farther west than Iowa, while 22 distinct species are now recognized from the western United States. Since none of these are mentioned in the book, does not the author's statement imply that in his opinion *Tamias striatus* ranges across the continent and that all of the 23 species of chipmunks are now conceded to be one and the same?

In his reply to my criticism he attempts to put himself right by stating that the early naturalists 'made several distinct species, so-called, of what are now conceded to be only geographical varieties of the single species *Tamias striatus*.' But here he falls into another error, as he will himself discover if he attempts to hunt up the 'several distinct species' he imagines the early naturalists tried to make of this animal.

If the author had ever seen young opossums '*soon after they are born*,'—tiny, naked, helpless, blind, embryonic things, each clinging to a teat in the mother's pouch, where they are carried for a long period before sufficiently developed to even peep out of the pouch—he would hardly have ventured to assert that at this period they go about on the mother's back, clinging to her tail. The author implies that my criticism of his antiquated scientific names is based on his avoidance of 'trinomial or other novelties of nomenclature more recently introduced.' In this he is greatly mistaken, as a few examples will show. And it might be added, in spite of his remarks against the use of technical names in popular books, that he has himself, in the book in question, used the following, and all of them erroneously: *Hesperomys*, *Arvicola*, *Urotrichus*, *Synetheres*, *Sorex cooperi*, *Castor fiber*, *Canis lupus*, *Scapanus breweri* and others.

The trouble with the book, as a whole, is that it contains altogether too many loose and inaccurate statements. A book for 'popular educational reading' ought, above all things, to be reliable and to show a groundwork of scientific accuracy.

Vernon Bailey.

Washington, D. C., January 21, 1898.

SCIENTIFIC LITERATURE.

Traité de Zoologie Concrète.—La cellule et les protozoaires. Delage et Hérouard, Paris. 1896. 527 pages; 870 illustrations.

This volume, the first of a series to be published by Delage and Hérouard, inaugurates a new departure in zoological text-books. The authors point out that the usual text-books (German and English) are not sufficiently definite and that the student, especially a beginner, has extreme difficulty in getting a mental picture of the animals which comprise any specific group. They maintain that the ordinary text-book, in dealing with such a group, introduces the subject by a few pages of comparative anatomy. The various organs and systems of organs are described for the group as a whole, but independently of any given animal, while exceptions to the common type are only casually mentioned. This introductory section is usually followed by an enumeration of the sub-divisions of the group and each subdivision is then treated in the same way as the group, beginning with the comparative anatomy, which is still vague and impersonal so far as the specific forms are concerned, and ending with a very short description of one or two characteristic genera. Nowhere in the chapter is any one animal completely described, and the student is confused by the variety of forms casually mentioned and bewildered by the numerous exceptions. Our authors regard such a text-book as 'abstract,' dealing neither with comparative anatomy nor systematic zoology but falling weakly between the two. In presenting their own 'concrete' zoology their aim is to avoid the evil above mentioned and to leave in the mind of the reader a complete mental picture of the structure of some type specimen of each group.

To take the place of the introductory sections of the usual text-books, they give a complete description of all the parts which make up a type specimen of the class or order in question. For this type specimen either some one form is chosen which represents the average of the group or an ideal form is created from the imagination. Such a form, whether imaginary or real, makes what the authors call the *morphological type* (Type morphologique). The description of this type specimen is very complete, comprising morphology, physiology, reproduction, regeneration, etc., while copious footnotes give further and more detailed information concerning special parts or historic connections. The description of the morphological type is followed by more condensed descriptions of the common genera, while the forms which are closely related are enumerated in footnotes. By describing so many they avoid exceptions to statements and so make the way clear to the student for every form or group of forms he reads about. Five hundred and thirty genera are actually described in the text, while half that number at least are mentioned in footnotes. Each genus is accompanied by a figure, sometimes colored, in which all of the parts are shown.

Fifty-eight pages are devoted to a general consideration of the cell and its functions. Here the structure, chemical composition and physiology of the cell, including nutrition, reproduction, fertilization, etc., are described in a general way, while extended footnotes give the main points on controverted questions in cellular biology. In this portion Delage follows pretty closely the lines of his own cytological researches, and when he deviates from them he is not always happy in his guide. For example, in his extended review of Fol's principle of the Quadrille of the Centers it is difficult to see why he ignores Wilson's complete disproval, and while cognizant of Boveri's and Mathew's work on the subject comes to the conclusion that Fol may still be right.

By far the largest part of the volume is devoted to the Protozoa (470 pages), and here we find their text-book plan completely worked out, although the simplicity of structure of the Protozoa gives little opportunity for testing the value of their color scheme, according to which the various organs are depicted in specific colors.

In the classification adopted the Protozoa are divided into the usual four classes—Rhizopoda, Sporozoa, Flagellia and Infusoria. The further subdivisions are only occasionally different from the usual classification. The authors follow Lankester in giving to the Mycetozoa the same taxonomic value as the Helioza and

Radiolaria. To be consistent, they should follow Haeckel, who has shown conclusively that the same reasoning which draws the Mycetozoa into the protozoan group would also draw the bacteria and fungi. This, however, they decline to do, and their classification of the Rhizopods is thereby weakened. The difficulty might have been avoided by introducing the questionable forms in an appendix under some name indicating their affinities to the plants. The same criticism might apply to their order Phytoflagellidæ. It is of value to show the connection of these plant-like forms to the Rhizopods and Flagellates, if for no other reason than to show the possible polyphylletic origin of Protozoa from Protophyta, but to make them equivalent to the well-defined animal groups seems to be a taxonomic error.

In classifying the Sporozoa the authors have left the beaten track and have taken advantage of the recent works of Labbé, Schneider and innumerable other investigators of this unfamiliar group to produce a new and apparently trustworthy classification in which the adult form is taken as the basis for the two main subdivisions—the Rhabdogeniæ (in place of Labbé's Histosporidæ and Cystosporidæ) and the Amœbogeniæ (equivalent to Bütschli's Myxosporidia).

One feature of the book which may be open to criticism is that nearly all of the figures taken from various special works are modified in some way to conform to the plan of schematization, and the reader is left with a feeling of uncertainty as to how much is real and how much imaginary, and he natuarally questions the degree of accuracy with which the authors draw the line between the two.

Another and a more important criticism touches the plan of presentation which is to be followed throughout the series. While there is undoubtedly much of value in the idea of their 'concrete' zoology for teaching purposes, there are important reasons why the method they adopt cannot give complete satisfaction. For example, one cannot resist a feeling of disapproval upon seeing an *Amœba proteus* described and pictured with the long reticulate and anastomosing pseudopodia of the Foraminifera in addition to its own lobose type; nor, indeed, a 'hypotrichous ciliate with the musculature of a heterotrichous form.' Such a method may be very successful in forcing upon the student a general idea of the group described, but the picture which he carries away with him may be of some form which does not actually exist in nature, while with that mental picture he carries a number of others which show deviations from the morphological type. It may be asked, then, if the confusion of pictures which the student gets is not as bewildering to him as the confusion of facts and exceptions in the 'abstract' type of text-book?

Finally, this work, although of undoubted value for teachers and specialists, is designed as a text-book for beginners, but, putting aside all considerations of method and merit, the mere size of any zoology which begins with 470 pages on the Protozoa and which promises to fill a large number of volumes is out of reach of the student, and he must continue to seek a text-book, probably of the 'abstract' type, which is condensed, simple, interesting and scientifically accurate.

GARY N. CALKINS.
DEPARTMENT OF ZOOLOGY,
COLUMBIA UNIVERSITY, NEW YORK,
December, 1897.

Sleep: Its Physiology, Pathology, Hygiene and Psychology. By MARIE DE MANACÉÏNE (St. Petersburg). Contemporary Science Series. Imported by Charles Scribner's Sons, New York. 1897. Pp. 335.

This work, already published in Russian and French, now appears in English, enlarged and revised by the author herself. It is a brief and somewhat popular summary of the best that is now known about the physiology, pathology, hygiene and psychology of sleep. The author's own investigations supplement a very wide range of reading on the subject. A classified bibliography enumerates about 550 books and articles pertaining more or less directly to sleep.

The one constant physical accompaniment of sleep is arterial, particularly cerebral, anæmia, with venous congestion, particularly congestion of the vessels of the skin, with dilatation of the arms and legs. The plethysmographic experi-

ments of Patrizi, Hill, Mosso and others are reviewed. The internal organs, stomach, kidneys, etc., may be in full activity during profound sleep. As regards the nervous system the inactivity is found in the centers rather than in the nerve and cord. The brain is in a collapsed, pale condition. The special senses may any of them be active, while walking, talking, and other movements are not incompatible with sleep. Even the brain may be active in some of its parts.

The various theories as to the cause of sleep are discussed and criticised. The vasomotor theories find sleep to be caused directly by the withdrawal of the blood from the brain, or indirectly by the relaxation of tone in the vasomotor center controlling the skin vessels, producing dilatation of the latter and anæmia at the centers. The chemical theories attribute sleep to the impoverishment of oxygen in the brain, or to the poisonous presence of carbonic acid or of leucomaines. Some recent histological theories of sleep explain it by assigning certain amœboid characters to the cerebral cells or to the cells of the neuroglia, the retraction of the ramifications of these cells resulting in isolation and inactivity of the nervous elements. In place of any of these theories the author herself very naively substitutes a psychical theory based upon the formula: Sleep is the resting time of consciousness. Hence we notice that those in whom consciousness is feebly developed, savages, infants, less cultured adults, require more sleep than others.

Under pathology, the writer treats of insomnia, syncope, excessive sleep, hibernation, narcolepsy, catalepsy, hypnosis, latah and somnambulism. In all of these the discussion is brief and presents nothing striking. Under hygiene, attention is called to the dangers of too much sleep to persons of all ages. In children it develops the vegetative life of the organism at the expense of the central nervous system. In boys and girls it is apt to lead to albuminuria. In adults it enfeebles the brain. Likewise, the half-waking state, hypnosis, the use of alcohol or narcotics, are all injurious, as they tend to produce an enfeebled consciousness. We should rise late in winter and early in summer. In the case of children perfect uniformity in the time of retiring and rising should be avoided.

Under the psychology of sleep, dreams are treated at some length, as to their classification, causes and peculiarities. The strangeness of dreams, as well as the criminal nature that they sometimes assume, is accounted for by their atavistic character. In dreams our personal, fully developed consciousness is asleep, while latent tendencies transmitted by our farthest ancestors tend to revive. "A good and peaceful man may awake in horror with forehead bathed in sweat from a dream in which he has been transported into some strange and antipathetic environment in which he has committed a barbarous and cruel deed, not altogether abnormal, but fully possible in the far past of humanity."

G. T. W. PATRICK.

UNIVERSITY OF IOWA.

SOCIETIES AND ACADEMIES.

NEW YORK ACADEMY OF SCIENCES—SECTION OF BIOLOGY—MEETING OF JANUARY 10, 1898.

PROFESSOR OSBORN spoke as follows on the Origin of the Mammalia: Huxley's hypothesis (1880) deriving the Hypotheria or Promammalia from ancient Amphibia contrasts with Cope's (1884), which substituted carnivorous reptiles of the Pelycosaur type included in his order Theromora. Baur (1886) placed the Theromora as a parallel phylum with the mammalia springing from Sauromammalia of the Permian. Osborn (1888) proposed the Protodonta as archaic mammals transitional to reptiles, and later (1893) adopted Baur's views as to the Theromora. More recently Baur has removed the Pelycosauria from the Theromora entirely, and thus speculation by the late Professor Cope, Baur and Osborn as to the origin of mammals turns back to the true Theromora, namely, the *Dicynodontia* and *Theriodontia* of Owen, a group which Professor H. G. Seeley has described in his numerous memoirs. Among these Permian reptiles of South Africa we find a remarkable assemblage of characters which comparative anatomy and paleontology have led us to anticipate in the hypothetical promammal. Osborn (1888 and 1893) described the probable

dental and mandibular characters of the promammal, and from the investigations of Baur, Howes, Hubrecht, Beddard, Albrecht and others are derived, in the skeleton and soft parts, other characters which are largely amphibian.

A comparison of the *Dicynodontia*, *Theriodontia* and *Gomphodontia* (Seeley) shows that, while widely separated in dental characters, these reptiles are closely united in numerous osteological characters, which, in turn, distinguish them from all other reptiles, the most striking regions being the palate, zygomatic arch, expanded squamosal and correlated reduction of the quadrate, and the complex structure of the occipital condyle. Owen's definitions are too narrow for this group, which appears to be embraced only in Cope's larger definition of the Theromora. As is well known, the *Dicynodontia* throughout the skeleton abound in mammalian characters, and in the skull exhibit a combination of special adaptations to the greatly developed canine teeth with persistent reptilian and promammalian characters. The term *Theriodontia* should be restricted, according to Owen's original definition, to carnivorous types, such as *Cynognathus*, with triconodont molar teeth and typical promammalian dental formula. The characters of the skull, teeth, vertebræ, pectoral and pelvic arches and limbs on the one side show the affinities of these animals to the Dicynodonts, and, upon the other, make them appear prophetic of the Jurassic *Triconodonta*. The Triassic *Protodonta* are quite as primitive in dentition, but different so far as known in that the jaw consists of a single bone. The third group, or *Gomphodontia*, Seeley, embraces herbivorous types with grinding teeth of multitubercular and rudely tritubercular pattern. The latter fact is of great significance in the support it apparently lends to Osborn's hypothesis that the multituberculates are of tritubercalate origin. In cranial characters these animals are as similar to the Theriodonts as they are dissimilar in dental characters, and since they include the genus *Tritylodon*, which was formerly placed among the Multituberculata, it appears possible that we have here a phylum more or less remotely related to the very ancient Mesozoic phylum of Multituberculata.

Summing up the Theriodont characters we find promammalian resemblances both in the form and formulæ of the teeth; in the terminal position of the anterior nares and structure of the palate; in the posterior expansion of the nasals; in the main infratemporal or zygomatic arch; in the great development of the squamosal and reduction of the quadrate; in the paired occipital condyles; in the intercentra of the cervical vertebræ; in the suturally united cervical ribs; in the intervertebral anterior dorsal ribs; in the Monotreme type of scapular arch (excepting, perhaps, the epicoracoid united by suture with metacoracoid); in the prescapular spine; in the powerful deltoid crest, large entepi- and ectepicondyles and entepicondylar foramina of the humerus. The limb and pelvic structure is evidence of a musculature similar to that of the hypothetical Promammalia, and of a body well raised above the ground and quadrupedal in position. As persistent reptilian characters may be cited the separate prefrontals, postfrontals and postorbitals and separate quadrate, which, according to Albrecht, is a reversional character in the Mammals; the separate transversum; distinct prevomer and complete pterygoquadrate arcade; the prominent basioccipital element; the separate elements of the lower jaw, and finally as adaptive or specialized characters are the several peculiar features in the back, skull and other parts of the skeleton. In conclusion, it appears that these true *Thermora* have the geological age required for the ancestors of the Mammalia. They are the only class of reptiles which exhibit mammalian affinities. They anticipate in the most surprising manner the dental structure of the ancient *Triconodonta* and *Multituberculata*. A most striking difference is found in their size, which far exceeds that of the oldest undoubted Mammals. This and certain specializations of structure bar any of the known *Thermora* from the ancestry of the earliest mammals, but do not preclude the existence of very small, unspecialized forms, which may have given rise to the oldest mammalian types. The existence of Amphibian structures, as observed by Hubrecht and others, in the placentation and soft anatomy of the mammals may be explained by the supposition that these Thermora retained certain Amphib-

ian structures from the ancestral Stegocephalia, which they transmitted to their descendants. The paired occipital condyle, however, upon which Huxley laid so much emphasis, is probably of secondary origin in this group, and not of direct derivation from the paired condyle of the Amphibians.

Dr. F. M. Chapman described the various types of vegetation and the altitudinal distribution of birds along the lines of the railroads running from the coast at Vera Cruz into the tablelands of the interior.

Professor F. E. Lloyd described the abnormal assimilative leaves produced by hypertrophy of scales on shoots of *Pinus ponderosa* after pruning of staminate shoots. The scales which subtend the fascicles so-called are the morphological equivalents of leaves.

Similar abnormal leaves are produced from the stump after cutting down the trees in certain species of Pine (*e. g.*, *Pinus rigida*). These have been regarded as identical with the primary leaves of the seedlings. The comparison of the hypertrophied scales under discussion with the primary leaves of species of Pine studied by Daguillon shows that they differ in certain details, and that they approach in structure to the *Abies* type of leaf which has peripheral ducts and double vascular bundles. The leaf of *Pseudotsuga* comports with this type, and the speaker suggested that the Pines may have been derived phylogenetically from a generalized form represented best among living genera by the genus *Pseudotsuga*, which combines the characters of *Abies* and *Picea* to a considerable degree. The exsert bracts are intermediate between these two genera, *Abies* and *Picea*, while the large seminiferous scales correspond more nearly to *Abies*.

These abnormal leaves of *Pinus ponderosa* must be regarded as atavistic, and are believed to be of pronounced value in the study of the phylogeny of the group.

GARY N. CALKINS,
Secretary of Section.

NEW YORK ACADEMY OF SCIENCE—SECTION OF GEOLOGY, JANUARY 17, 1898.

THE meeting opened with a paper by Mr. Arthur Hollick, entitled 'Further Notes on Block Island; Geology and Botany.' Mr. Hollick gave a summary of his work done on Block Island in July, 1897, and particularly of his success in tracing eastward from Long Island the Amboy clays which had previously been determined by paleontological evidence on Staten Island, Long Island and Martha's Vineyard. Something like fifteen species of Middle Cretaceous flora, nine of them typical of the Amboy clay, have been found. Mr. Hollick then classified the existing flora of theIsland physiographically into that of the hills, peat bogs, sand dunes and beaches, salt marshes and salt water. In the course of his work he added to the already published lists something like twenty-four new species, though it is not considered that this, by any means, completes the list of possible species that might be found in the spring. The flora, as a whole, is distinctly that of a morainal country, and its nearest analogue is that of Montauk Point.

Mr. Hollick then offered some suggestions to account for the present peculiar flora of the Island, and particularly for the absence of certain species that would be expected, and showed that two features are to be taken into consideration, the geological and the human. Block Island is the only part of the terminal moraine along the New England coast which does not have accompanying the moraine a certain amount of plain land, which would naturally allow a variety in the flora. It is presumable that Block Island also has been practically separated from the rest of the continent by a deep channel of more than twenty fathoms for a considerable time, and that even before the last depression of land the Island was connected to the mainland merely by a small peninsula, and hence the diversity of the flora as compared with the continent, because of the length of separation. The speaker also mentioned extensive archæological discoveries on the west shore of the Island, and gave a list of the shells and implements discovered in several of the kitchen middens, and also of the bones. of animals brought to light in the old fireplaces in the sand dunes. He made particular mention, also, of the great number of Littorina, the common periwinkle of Europe, which has never before

been announced from Block Island. The paper was discussed by Professors Lloyd and Martin.

The second paper of the evening was by the Secretary, entitled 'Scientific Geography in Education.' The speaker brought out the point that geography work may be classified into three divisions—that for the common schools, the secondary schools and the universities—and outlined briefly a few suggestions as to how the subject-matter might be treated scientifically in each of the groups, and the dependence of each group upon the others. He paid particular attention to the difficulties of securing scientific work in geography in the grade schools, and to the fact that the present work is extremely unsatisfactory in most of our schools, probably because of the lack of inspiration, owing to the neglect of the subject hitherto in universities of the country. The paper was illustrated by a series of cheap and easily procurable maps that may be used for scientific geography work in either of the groups mentioned.

The meeting then closed with a few remarks by the Chairman in reference to the famous classic entitled 'Lithographiæ Wircenburgensis ducentis lapidum figuratorum, a potiori insectiformium prodigiosis imaginibus exornatæ, specimen primum,' written by Dr. Beringer and published in Würzburg in 1726. Professor Kemp summarized the work of the author in attempting to explain a great collection of pseudo-fossils from a theological standpoint, the fossils having previously been made by some practical jokers and buried in the rocks for the author to find.

RICHARD E. DODGE,
Secretary.

SUB-SECTION OF ANTHROPOLOGY AND PSYCHOLOGY.

AT the regular meeting of the New York Academy of Sciences at 64 Madison Avenue, Monday evening, January 24th, fourteen new names were proposed for membership. This is evidence of the increased interest being awakened in the Academy by the active efforts of President Stevenson. The hope was expressed that the number of members might soon be raised to five hundred.

The principle paper of the evening was presented by Mr. E. L. Thorndike, of Columbia University. He gave an account of a long series of interesting experiments on comparative psychology. These experiments were made upon cats, chickens, dogs, monkeys and other animals and were supplemented by the experience of professional animal trainers.

Cats were placed in boxes with doors so arranged that they could be opened from the inside in various ways, in one set of experiments by pressing a latch, in another by pulling a cord, by pulling a hook attached to a cord, or by turning a button. Again the arrangement was more complicated and two or three separate movements had to be combined in order to release the door and let the animal out to reach the fish placed outside the cage. Curves were given showing the rate at which the kittens learned the various tricks, the time taken to get out becoming gradually shorter.

The trick was always learned by accident; one lucky hit would prepare the way for another. There was no trace of rational inference. Seeing another animal do the trick a hundred times was no help. Nor was it possible to teach the trick by taking the kitten's paw and putting it on the latch and so opening the door, no matter how often it was repeated.

A habit once formed artificially will overpower natural instincts. A chicken that had been compelled to jump from a box to the floor in a roundabout way by a cardboard placed in its way felt unable to jump down to its food directly when the card was taken away.

The second paper was presented by Mr. H. I. Smith, of the Museum of Natural History. He gave an account of the archæological work which he did in British Columbia during the summer. He was the third member of the Jesup expedition, with Dr. Boas and Dr. Farrand. The work of the expedition has already been described in SCIENCE.

Dr. Livingston Farrand, of Columbia University, presented a brief report of the meeting of the American Psychological Association held at Cornell during the holidays.

CHARLES B. BLISS,
Secretary.

CHEMICAL SOCIETY OF WASHINGTON.

AT the fourteenth annual meeting, held January 13th, the following officers were elected for the ensuing year, viz.: President, Henry N. Stokes; Vice-Presidents, Peter Fireman, H. Carrington Bolton; Secretary, William H. Krug; Treasurer, W. P. Cutter; Executive Committee, the above and Charles E. Monroe, E. A. de Schweinitz, Wirt Tassin, W. F. Hillebrand.

V. K. CHESNUT,
Secretary pro tempore.

BIOLOGICAL SOCIETY OF WASHINGTON—285TH MEETING, SATURDAY, JANUARY 15.

THE major part of the evening was devoted to 'A Symposium on Recent Additions of Our Knowledge of the Cell,' the subject being introduced by Dr. Frank Baker, who gave a brief *résumé* of the successive discoveries in regard to the structure of the cell, touching on the theories of the alveolar and filar structures of the cytoplasm and dwelling at some length on the changes which take place in the nucleus during cell division.

Messrs. David G. Fairchild, Herbert J. Webber and Walter T. Swingle, who followed, presented the topic chiefly from a botanical standpoint, showing that the processes of nuclear and cell division were much more varied in plants than among animals, and might be very different, even taking place without the presence of a centrosome.

F. A. LUCAS,
Secretary.

BOSTON SOCIETY OF NATURAL HISTORY.

THE Society met December 15th, one hundred and five persons present.

Professor W. M. Davis, with the aid of a series of lantern slides, gave a graphic account of excursions from the Atlantic to the Pacific. Some of the prominent physiographic features of parts of New England, Niagara, the Lake Superior Region, the Lake of the Woods, Lake Simcoe, the Black Hills, the Canadian Rockies and portions of the country along the Northern Pacific Railroad were described and illustrated.

A general meeting was held January 5th, forty-two persons present.

Mr. Frank Russell read some notes upon the Athabascan Indians, as observed in the neighborhood of the Great Slave Lake, on the Barren Ground of Canada. The men devote themselves to hunting, traveling in canoes and on snow shoes; the women are hard workers and, in addition to all the household duties, prepare the skins and make the garments. Personally the men are more cleanly than the women. Tattooing is not now practiced, and, under the influence of the Roman Catholic missionaries, polygamy has been abandoned; the Athabascans are Christians and Catholics. Mr. Russell also described many Athabascan songs, their music, the methods of camp making, and the celebration at Easter, and closed with a series of lantern views illustrating the physical type of the tribe, their dwellings and some of their habits and customs.

Mr. John Murdoch said that the canoes, as shown by Mr. Russell, were similar to those used on the Yukon.

SAMUEL HENSHAW,
Secretary.

NEW BOOKS.

Text-book of Physical Chemistry. CLARENCE L. SPEYERS. New York, D. Van Nostrand Co. 1897. Pp. vii+224. $2.25.

The Mathematical Theory of the Top. FELIX KLEIN. New York, Charles Scribner's Sons. 1897. Pp. 74.

A Short Handbook of Oil Analysis. AUGUSTUS H. GILL. Philadelphia and London, J. B. Lippincott Co. Pp. 139.

Chapters on the Natural History of the United States. R. W. SHUFELDT. New York, Studer Brothers. 1897. Pp. 472+Index.

A Primer of Psychology. EDWARD BRADFORD TITCHENER. New York and London, The Macmillan Company. 1898. Pp. xvi+314. $1.00.

A Description of Minerals of Commercial Value. D. M. BARRINGER. New York and London, Chapman & Hall, Ltd. 1897. Pp. 168.

A. Ecker's and R. Wiedersheim's Anatomie des Frosches. Revised by ERNST GAUPP. Braunschweig, Friedrich Vieweg und Sohn. 1896–7. Pp. xiii+229 and ii+234.